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ANNALS
OF
MEDICINE,
FOR THE YEAR 1798.

EXHIBITING A CONCISE VIEW OF
THE LATEST AND MOST IMPORTANT DISCOVERIES
IN MEDICINE AND MEDICAL PHILOSOPHY.

BY
ANDREW DUNCAN, SEN. M. D.
AND
ANDREW DUNCAN, JUN. M. D.
FELLOWS OF THE ROYAL COLLEGE OF PHYSICIANS,
EDINBURGH.

> In curationibus morborum, illud generaliter deſiderari reperio, quod Medici hujuſce ætatis, licet generales intentiones curationum non male perſequantur, particulares tamen medicinas, quæ ad curationes morborum ſingulorum, proprietate quadam ſpectant, aut non bene norunt, aut non religioſe obſervant. BAC. DE AUG. SCIENT.

> Parvula (nam exemplo eſt) magni formica laboris
> Ore trahit quodcunque poteſt, atque addit acervo.—HORAT.

VOL. III.

EDINBURGH:
PRINTED BY J. PILLANS & SONS,
FOR WILLIAM MUDIE, SOUTH BRIDGE; G. G. & J. ROBINSON,
LONDON; & FRED. PERTHES, HAMBURGH.
1799.

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ANNALS
OF
MEDICINE,
FOR THE YEAR 1798.

SECT. I.
Analyſis of Books.

I.

Medical Reports on the Effects of Water, cold and warm, as a remedy in Fever and Febrile Diſeaſes, whether applied to the ſurface of the Body, or uſed as a drink; with Obſervations on the nature of Fever, and on the Effects of Opium, Alcohol, and Inanition. By James Currie, *M.D. F.R.S. Fellow of the Royal College of Phyſicians,* Edinburgh, *and Phyſician to the Liverpool Infirmary.* *8vo.* London, 1797.

IN an addreſs to Sir Joſeph Banks, prefixed to this treatiſe, the ingenious author gives a ſhort account of the views with which it was written, and of the circumſtances which gave riſe to its publication.

He tells us, that about eighteen years ago, when he was at Edinburgh, it fell to his lot to write a paper on the influence of cold on the living body, for a ſociety of ſtudents. From his reſearches at that time, he ſoon diſcovered, that the accounts given of the temperature of the ſyſtem under diſeaſe, by the moſt approved authors, were not founded on exact meaſurements of heat, but on the ſenſations of patients, or attendants. Impreſſed with the belief, that, till more accurate information be obtained, no certain progreſs can be made in the treatment of thoſe diſeaſes in which the temperature is diminiſhed or increaſed, he has ſince that time obſerved and recorded ſuch facts as related to the ſubject, with the intention of preſenting his obſervations to the public.

The fruits of Dr Currie's induſtry and genius on this ſubject, are now delivered to us in the preſent volume; and we may eaſily venture to aſſert, that it contains much important information, particularly with reſpect to the practice of medicine.

With the fever which had prevailed in the Weſt Indies and America, ſcience ſeemed in a great meaſure to have contended in vain. New means of oppoſing it, therefore, were not only juſtifiable, but requiſite. At this time, the ſucceſs of the nitric acid, in lues and hepatitis, was communicated to the public, by Mr Scott of Bombay. The firſt trials which Dr Currie made, perſuaded him, that though the ſucceſs of this remedy may have been exaggerated by a warm imagination, it aſſuredly did ſucceed in certain caſes of theſe diſeaſes. In the Weſt Indies, mercury had been much employed againſt the contagious fever, and ſeemed, upon the whole, the moſt approved remedy. And ſince the nitric acid appeared to be a ſubſtitute for mercury in other caſes, it ſeemed reaſonable to try its effects in this fatal fever.

Reflection on this ſubject, turned Dr Currie's views to other means of oppoſing this peſtilence. Ablution with cold water had been for ſeveral years ſucceſsfully employed, both in hoſpital and private practice, not only by Dr Currie himſelf, but alſo by his friends and colleagues, Dr Gerard and Brandreth; and in 1791, a general ſtatement of the advantages reſulting from it, communicated by the laſtmentioned gentleman, to Dr Duncan, had been publiſhed in the Medical Commentaries of Edinburgh. Reflecting on theſe circumſtances, and expoſed by his ſituation to the reiterated ſounds of death, from the weſtern world, he determined no longer to delay communicating to the public a full account of this method of treating fever. And the work now before the public is the fruit of this determination.

Dr Currie introduces his ſubject, by giving a ſhort narrative of the ſucceſsful treatment of ſome caſes of fever, by the ablution of the patient with cold water, as publiſhed in the London Medical Journal, for the year 1786, by Dr William Wright, formerly of the iſland of Jamaica. Dr Wright firſt tried this practice on himſelf, in his paſſage from Jamaica to Liverpool, in September 1777. From attention to a ſick man on board the veſſel, Dr Wright caught the contagion. After employing other remedies in vain, he reſolved to put in practice on himſelf, what he had often wiſhed to try on others in fevers of a ſimilar nature. Having therefore given the neceſſary directions, he ſtript off all his clothes, and threw a ſea-cloak looſely about him, till he got upon deck, when the cloak alſo was laid aſide. Three buckets of ſalt water were then at once thrown upon him. The ſhock was great, but he felt immediate relief. His headach and other pains inſtantly abated, and a fine glow and diaphoreſis ſucceeded. Towards evening, however, the febrile ſymptoms threatened a return, and he had again recourſe to the ſame method, with the ſame good effect. He now took food with ſome appetite, and for the firſt time had a ſound night's reſt. For two days longer, the ſame practice was repeated. And he then found himſelf totally free from the diſeaſe.

Dr Currie next gives us a ſhort hiſtory of a contagious fever which made its appearance in the Liverpool Infirmary, in December 1787. Before its progreſs could be arreſted,

reſted, ſixteen perſons were affected, of whom two died. On this occaſion, Dr Currie uſed, for the firſt time, the affuſion of cold water, in the manner deſcribed by Dr Wright. He began by trying it in two caſes, the one in the ſecond, the other in the fourth day of the fever. The effects correſponded exactly with theſe mentioned by Dr Wright to have occurred in his own caſe. Thus encouraged, the remedy was employed in five other caſes, and the whole of theſe patients recovered.

From that time, Dr Currie has conſtantly wiſhed to employ the affuſion of cold water in every caſe of the low contagious fever, in which the ſtrength was not already much exhauſted. And he has preſerved a regiſter of an hundred and fifty-three caſes, in which the cure was chiefly truſted to this remedy. Of theſe, ninety-four occurred in the hoſpital, in the four years ſubſequent to the period already mentioned; twenty-ſeven in private practice; and thirty-two in the 30th regiment of foot, when quartered in Liverpool, in the year 1792. Of late, he has not thought it neceſſary to regiſter all the caſes in which this remedy has been employed. Being ſatisfied of its extraordinary ſucceſs, he has been chiefly occupied in aſcertaining the precautions neceſſary in uſing it. In the work before us, therefore, he endeavours to digeſt the reſults under a few diſtinct heads, ſupporting and illuſtrating each propoſition by an ample detail of caſes.

But before cloſing his preliminary obſervations, he gives a ſhort account of the riſe, progreſs, and termination of the contagious fever, which occurred to the 30th regiment at Liverpool, and which he thinks, while it ſupports, in a ſtriking manner, the doctrines he endeavours to eſtabliſh, affords alſo important inſtruction, as well as encouragement, to thoſe whoſe duty may call them to oppoſe the progreſs of contagious fever in ſimilar ſituations. Previous to the arrival of the 30th regiment at Liverpool, the general guard-room at the fort had been uſed as a place of confinement for deſerters. It was extremely cloſe and dirty, and under it was a cellar, which in winter had been full of water. This water had been now half evaporated, and from the ſurface iſſued offenſive exhalations.

In a narrow dark unventilated cell of the guard-room, it was uſual to confine ſuch men as were ſent to the guard for miſbehaviour. About the beginning of June, ſeveral men had been ſhut up in this place, on account of drunkenneſs; and ſuffered to remain there twenty-four hours, under the debility which ſucceeds intoxication. The typhus, or jail-fever, made its appearance in two of theſe men, and ſpread with great rapidity. Ten of the ſoldiers labouring under this epidemic, were received into the Liverpool Infirmary; and the wards allotted to fever could admit no more. The contagion continuing its progreſs, a temporary hoſpital was fitted up at the fort, and Dr. Currie was requeſted to give his aſſiſtance there to the ſurgeon of the regiment.

In two low rooms, each about fifteen feet ſquare, he found fourteen patients labouring under fever. The ſymptoms of this fever were very uniform. In every caſe there was more or leſs cough, with mucous expectoration. In all thoſe who had ſuſtained the diſeaſe eight days and upwards, there were petechiæ on the ſkin. In ſeveral there were occaſional bleedings from the noſtrils, and ſtreaks of blood in the expectoration. The debility from the commencement was conſiderable. The pulſe varied from 100 to 130 ſtrokes in the minute. The heat roſe in one caſe to 105 of Fahrenheit; but was in general from 101 to 103. And towards the latter ſtages of the diſeaſe, it was ſcarcely above the temperature of health. Great pain in the head, with ſtupor, pervaded the whole; and in ſeveral inſtances, there occurred a conſiderable degree of low delirium.

Their firſt care was to ventilate and clean the rooms; the ſecond, to waſh and clean the patients themſelves. This was done by pouring cold ſea-water over the naked bodies of thoſe whoſe ſtrength was not greatly reduced, and whoſe heat was ſteadily above the temperature of health. In thoſe advanced in the fever, whoſe debility was of courſe great, this treatment was not ventured upon. But the whole ſurface of the body was ſpunged with tepid vinegar; a practice which, Dr Currie obſerves, is in every ſtage of fever ſalutary and refreſhing.

They next endeavoured to ſtop the progreſs of the infection. Every means of purifying the guard-houſe, by waſhing and ventilation, being found ineffectual, it was ſhut up, and a temporary ſhade erected in its ſtead. But ſtill the contagion proceeded. On the 13th of June, therefore, at Dr Currie's requeſt, the whole regiment was drawn up, and the men examined in their ranks. Seventeen were found with ſymptoms of fever upon them, who were readily diſtinguiſhed by their countenances being languid, and their whole appearance dejected with a dull red ſuffuſion of the adnata of their eyes. Theſe men were carefully ſeparated from the reſt, and immediately ſubjected to the cold affuſion, always repeated once, ſometimes twice a-day. In fifteen of the number, the contagion was extinguiſhed, but two went through the regular diſeaſe. On the ſame day the commanding officer, at Dr Currie's deſire, iſſued an order for the whole of the remaining part of the regiment to bathe in the ſea; and for ſome time they were regularly muſtered, and marched down at high water, to plunge into the tide.

Theſe means, Dr Currie thinks, had the effect of arreſting this epidemic. For, after the 13th of June, no perſon was attacked by it. It extended to fifty-eight perſons in all, of whom thirty-two went through the regular courſe of the fever, and in twenty-ſix the diſeaſe ſeemed to be cut ſhort by the cold affuſion. Of the former number two died. But both of them were men weakened by the climate of the Weſt Indies, and neither of them were ſubjected to the cold affuſion, the diſeaſe being far advanced before Dr Currie ſaw them.

The practice now detailed, Dr Currie thinks, may be followed with great eaſe, ſafety, and advantage, in hoſpitals, manufactories, and priſons, where the low contagious fever frequently originates. But, above all, he conſiders it as particularly applicable on ſhip-board, where the waters of the ocean conſtantly afford this remedy, and where it can be applied almoſt as eaſily as it can be procured.

Dr Currie next treats of the manner in which the affuſion of cold water ought to be uſed in fever. In the moſt common fever of England, the low nervous fever, as it is commonly called, the typhus of Dr Cullen, one exacerbation, and one remiſſion in twenty-four hours, ſeem generally obſervable. The exacerbation uſually occurs in the afternoon or evening, the remiſſion towards morning. The exacerbation is marked by increaſed fluſhing, thirſt, and reſtleſſneſs. If the heat of the patient be at ſuch times meaſured by the thermometer, it will be found to have riſen one or two degrees in the central parts of the body, and ſtill more on the extremities. The ſafeſt and moſt advantageous time for uſing the aſperſion or affuſion of cold water, Dr Currie tells us, is when the exacerbation is at its height, or immediately after its declination is begun; and this has led him almoſt always to direct it to be employed between ſix and nine o'clock in the evening. But it may, he affirms, be ſafely uſed at any time of the day, when there is no ſenſe of chillineſs preſent, when the heat of the ſurface is ſteadily above what is natural, and when there is no general or profuſe perſpiration.

If the aſperſion be uſed during the cold ſtage of the paroxyſm, the reſpiration is nearly ſuſpended; the pulſe, with great increaſe of frequency, becomes fluttering and feeble, and the patient ſeems to ſtruggle with the pangs of inſtant diſſolution. In ſuch circumſtances, Dr Currie has no doubt that the repeated affuſion of a few buckets of cold water would extinguiſh life. The remedy therefore ſhould never, he obſerves, be uſed when any conſiderable ſenſe of chillineſs is preſent, even although the thermometer, applied to the trunk of the body, ſhould indicate a degree of heat greater than uſual. Neither ought it, he tells us, to be uſed when the heat meaſured by the thermometer is leſs than natural, or even only equal to the natural heat, though the patient ſhould feel no degree of chillineſs. And it is alſo neceſſary to abſtain from the uſe of this remedy when the body is under profuſe perſpiration, eſpecially after it has continued for ſome time, and flowed freely. Under theſe reſtrictions, cold affuſion may, Dr Currie affirms, be uſed at any period of fever. But its effects, he tells us, will be more ſalutary in proportion as it is uſed more early.

In confirmation of theſe obſervations, Dr Currie relates eight caſes, ſelected from a great number, in which this practice was employed by him. Theſe eight afford illuſtrations of the different particulars already mentioned. But without detailing the whole, we ſhall here content ourſelves with relating the firſt and ſeventh, which are in ſome circumſtances the moſt remarkable.

On the 1ſt of January 1790, a nurſe in the fever-ward of the Infirmary having ſeveral patients under her care, caught the infection. She was ſeized with violent rigors, chillineſs, and wandering pains, ſucceeded by great heat, thirſt, and headach. Sixteen hours after the firſt attack, her heat at the axilla was 103 of Fahrenheit, her pulſe 112 in the minute, and ſtrong, her thirſt great, her tongue furred, and her ſkin dry.

Five gallons of ſalt water, of the temperature of 44 degrees, were poured over her naked body; and, after being haſtily dried with towels, ſhe was replaced in bed. When the agitation and ſobbing had ſubſided, her pulſe was found to beat at the rate of 96 ſtrokes in the minute, and in half an hour afterwards it had fallen to 80. The heat was reduced to 98 degrees by the ablution, and half an hour afterwards it remained ſtationary. The ſenſe of heat and headach were gone, and the thirſt nearly ſo. Six hours afterwards, ſhe was found perfectly free of fever, but a good deal of debility remained.

Small doſes of colombo were ordered for her, with a light nouriſhing diet; and for ſeveral days the cold affuſion was repeated at the ſame hour of the day. The fever never returned.

The ſeventh caſe here related, is that of F. G. a ſoldier of the 30th regiment, aged 33. He fell under Dr Currie's care on the 9th of June 1792, during the prevalence of the epidemic in that regiment, which has already been mentioned. He was in the 9th day of the diſeaſe. His pulſe 100, and feeble, his heat 104 degrees, his thirſt great, his tongue foul and black; frequent cough occurred, with ſtreaks of blood in the expectoration; and petechiæ appeared all over his body. His mind was at all times confuſed, and occaſionally he was completely delirious.

Dr Currie directed that his ſtrength ſhould be ſupported by a bottle of wine every day, with an equal quantity of gruel; that every night he ſhould take an opiate draught; and that a complete operation of his bowels ſhould be procured by a clyſter adminiſtered daily; and if this did not ſucceed, by a few grains of calomel. He alſo directed, that a bucket full of ſalt water ſhould be thrown over him immediately, and repeated according to circumſtances. In a few minutes after the affuſion, the heat was 98 degrees, the pulſe 98, his mind was more calm and collected: two hours afterwards, he had relapſed nearly unto his former ſtate; but the night was paſſed more tranquilly. The whole of this practice was continued with nearly the ſame reſult, till the 12th day of the diſeaſe, the affuſion having always been performed in the evening, and occaſionally at noon. The fever continued its uſual period; but on the 12th day, the heat having ſunk to its natural ſtandard, the cold affuſion was thenceforth omitted; the whole of his body was, however, ſpunged once or twice a-day with vinegar; and he was in a ſtate of convaleſcence on the 18th day from the firſt attack. This caſe is peculiarly intereſting, as it affords an example of the effects of cold affuſion, uſed in the more advanced periods of fever.

Dr Currie next points out ſome precautions requiſite in uſing cold affuſion, and illuſtrates them by caſes. He begins by relating a caſe which demonſtrates, that cold affuſion cannot be uſed with ſafety, during the cold ſtage of a febrile paroxyſm. A patient ſubjected to an intermittent fever was taken out of his bed ſhivering, his pulſe ſmall and frequent, his extremities cold. In this ſtate, brine was daſhed over him in the uſual manner; but not with the uſual happy effects. His breathing was for ſome minutes almoſt ſuſpended; his pulſe at the wriſt was not to be felt; the pulſations of the heart were feeble and fluttering; a deadly coldneſs ſpread over the ſurface; and when reſpiration returned, it was ſhort, irregular, and laborious. After the uſe of frictions on the ſurface, and particularly on the extremities, of a ſteady warmth, applied for ſome time to the ſcrobiculus cordis, and of cordials cautiouſly adminiſtered in ſmall quantities, the pulſe at the wriſt returned, but for ſome time it was exceſſively quick and feeble. He recovered, however, in the courſe of an hour, and it was found that the paroxyſm of fever had been extinguiſhed. But the circumſtances related were evidently full of danger, and they produced, Dr Currie tells us, much apprehenſion and uneaſineſs. The ſame remedy, however, was uſed in the hot ſtage of the enſuing paroxyſm, and with the uſual happy effects. Hence it appears, that although cold affuſion may be recommended in the hot ſtage of fever, with ſafety and advantage, yet it ought never to be forgotten, that an application of cold, which is ſafe in the violence of fever, is not ſafe when the fever is removed. Dr Currie has frequently uſed cold affuſion in the hot ſtage of the paroxyſm of intermittents, and almoſt always with the immediate ſolution of the fit. But he tells us, that in general, if no remedy be uſed in the intermiſſion, the fever returns at the uſual period.

The cold affuſion, Dr Currie obſerves, is not ſafe, after the ſweating ſtage of fever has continued for ſome time, and the body is paſſing through that cooling proceſs. This obſervation is alſo demonſtrated by a caſe, in which, though the happieſt effects were produced, the affuſion was not unattended with hazard.

In recommending, therefore, the affuſion of cold water as a remedy in fever, an expreſs exception is to be made againſt its uſe during the feveriſh chill, or after the perſpiration has begun to flow profuſely, and more eſpecially after it has continued to flow profuſely for ſome time. An exception is alſo to be made againſt its being employed in the latter end of fever, when the ſtrength is much exhauſted, and the heat is ſometimes lower than the temperature of health.

Under theſe reſtrictions, however, the affuſion of cold water may, Dr Currie aſſerts, be uſed with perfect ſafety in the low contagious fever of this country. In the firſt ſtages of fever, it appears very generally to cut ſhort the diſeaſe, almoſt inſtantaneouſly. And even when it fails in this effect, as is uſually the caſe when it is applied in the more advanced ſtages, it nevertheleſs moderates the violence of the ſymptoms, and ſhortens the duration of the fever.

After ſome remarks on a peculiar modification of fever, particularly diſtinguiſhed by a remarkable ſenſibility of the ſurface, as well as of hearing, taſte, ſmell, and touch, in which cold affuſion did not ſucceed, Dr Currie next proceeds to treat of the affuſion of cold and tepid water in ſmall pox. The great advantage which is experienced in this diſeaſe, by the admiſſion of cold air, naturally points out the external uſe of cold water, which, being a more powerful application, might be more particularly adapted to its more malignant forms. The reſult correſponded entirely with Dr Currie's expectations. And in proof of this, he relates two caſes, ſelected from a conſiderable number, in which he has witneſſed the happy effects of the affuſion of cold water in this diſeaſe.

The firſt of theſe was a caſe of inoculated ſmall-pox: The patient, an American gentleman, in the 24th year of his age, ſickened on the ſeventh day after the operation, and the eruptive fever was conſiderable. He had a rapid and feeble pulſe, a fœtid breath, with pain in the head, back, and loins. His heat roſe in a few hours to 107 degrees, and his pulſe beat 119 times in a minute. Dr Currie encouraged him to drink largely of cold water and lemonade, and threw three gallons of brine over him. He was in a high degree refreſhed by it. The eruption fever abated in every reſpect; an incipient delirium ſubſided; the pulſe became ſlower, the heat was reduced, and tranquil ſleep followed. In the courſe of 24 hours, the affuſion was repeated three or four different times at his own deſire. The eruption was of a favourable kind; and he recovered rapidly.

Where the eruptive fever does not abate ſufficiently on the admiſſion of cold air, the affuſion of cold water, Dr Currie thinks, may be reſorted to with confidence and ſafety; regulated, however, by the actual ſtate of the patient's heat, and of his ſenſation of heat. But in the confluent ſmall-pox, after the eruption is completely formed, he doubts whether this remedy can be uſed with advantage. This poſition is, he thinks, eſtabliſhed by the ſecond caſe he relates, which had a fatal termination, and which he here details minutely, as recording accurately the degree of heat in confluent ſmall-pox, which had not before been ſufficiently attended to.

The ſubject of this caſe was an American mariner, in the 23d year of his age. He came under Dr Currie's care on the 7th day of December, the third day of the eruption of ſmall-pox, the ſixth of the diſeaſe. His pulſe was 114, and feeble, and his heat 100, with great thirſt, headach, and other ſymptoms of confluent ſmall-pox. He was put upon a milk-diet, gentle mercurial purgatives were ordered from time to time, and an opiate every night at bed-time. The affuſion of cold water was directed in the uſual way. In ten minutes after the affuſion, the pulſe was 96, the heat 98 degrees, and the livor of the ſkin was much diminiſhed.

December 8th, noon, pulſe 96, ſoft and regular, thirſt gone, reſpiration ſlow and natural, heat 97 degrees. The affuſion was ordered to be repeated; ten minutes after, pulſe 84, and feeble, heat 84.

December 9th, pulſe 88, heat 93 degrees. The cold affuſion was not repeated in this very reduced ſtate of heat. The decoction of Peruvian bark was ordered, and a pint of wine daily in lemonade.

December 10th, pulſe 116, and full, heat 98 degrees, reſpiration ſtill eaſy, expectoration conſiderable and viſcid, thirſt leſs, eyes quite cloſed, head ſwelled; a complete union of the puſtules on the face. Bark and wine continued with the opiate at night.

December 12th, pulſe 118, heat 96. A bucketful of water, of the temperature of 92, was poured over him. He appeared refreſhed at the moment. Ten minutes after, pulſe 112, heat 94. Complained of being chilly. Reſpiration ſtill eaſy; free of pains, and his face fallen. Complained of his throat; a bliſter was applied to it all round.

December 13th, pulſe 118, heat 96, reſpiration ſtill free, but his throat is very ſore. Medicines were continued, but the affuſion of tepid water was not repeated.

December 14th, pulſe 138, heat 100 degrees. Reſpiration is now become laborious, and the expuition difficult. The throat is much ſwelled. He was frequently ſpunged with tepid water, and the medicines continued.

December 15th, unfavourable ſymptoms increaſed.

December 16th, vomiting came on, which was relieved by opium. His ſenſes and intellect remained acute to within an hour of his death, which happened at eight o'clock in the evening of this day.

In this caſe, the heat in the confluent ſmall-pox is accurately recorded from the period when the diſeaſe came under Dr Currie's care. In the ſtage in which the cold affuſion was firſt employed, the fever and heat were abating. After the ſecond affuſion, the heat ſunk below its natural ſtandard, and continued below it for ſome time, ſo that this remedy became inadmiſſible. It will at once be perceived, Dr Currie obſerves, that in a diſeaſe like this, the affuſion of cold water could only be uſeful during the eruptive fever. It is, he adds, during the eruptive fever, that the quantity of aſſimulation is determined, as well as its kind; and there is every reaſon to expect, that a diminution of this fever will diminiſh the quantity and meliorate the quality of the variolous eruption.

After ſome remarks on the affuſion of tepid water, as practiſed among the ancients, Dr Currie concludes the account of his experience reſpecting the employment of cold water in fever, with obſervations on the uſe of it, as a drink in that diſeaſe. Among the ancients, he obſerves, the internal uſe of cold water in ardent fevers, is recommended by Hippocrates, Celſus, Galen, and moſt of the celebrated phyſicians whoſe works have come down to us. Among the moderns, it has been ſtrongly recommended by Cardanus, Hoffman, and other eminent writers; but particularly by Dr Cyrillus of Naples, who has recommended it under the title of Diæta Aquea. But the propriety of giving cold water in fevers has been diſputed by Boerhaave, Van Sweiten, and other eminent moderns; ſo that, Dr Currie thinks, it may be upon the whole aſſerted, that the uſe of cold drink in fever is contrary to modern practice.

Dr Currie was naturally led to examine the effects of cold water as a drink in fever, from his experience of its effects as an external application. He made this examination with the thermometer in his hand, and with all the attention in his power; and from this examination, he draws the following reſults.

1. Cold water is not to be uſed as a drink in the cold ſtage of the paroxyſm of fever, however urgent the thirſt.

2. When the hot ſtage is fairly formed, and the ſurface is dry and burning, cold water may be drunk with the utmoſt freedom. Large draughts of it at this period are not only highly grateful, but they generally diminiſh the heat of the ſurface ſeveral degrees, and lower the frequency of the pulſe. By reducing the heat to the ſtandard at which perſpiration flows moſt freely, it brings the paroxyſm to a ſpeedier iſſue.

3. But after the perſpiration has become general and profuſe, the uſe of cold drink is ſtrictly to be forbidden.

Thus, then, he thinks we may ſafely adopt the ſame general rules, for the uſe of cold water in fever as a drink, that have already been laid down for its external application. It may be uſed as a drink with ſafety and advantage, at any time, when there is no ſenſe of chillineſs preſent, when the heat of the ſurface is ſteadily above what is natural, and when there is no general or profuſe perſpiration.

After ſome obſervations on the diſeaſe, which ariſes from drinking cold liquid after ſevere exerciſe, which Dr Currie aſcribes to its being taken at a time when the body is ſuddenly parting with its heat, he next proceeds to treat of the uſe of the cold bath in convulſive diſeaſes, and inſanity. Dr Currie is of opinion, that the chief benefit derived from the cold bath, in convulſive diſeaſes, depends on its being uſed in the paroxyſm of convulſion; that its efficacy conſiſts in reſolving or abating the paroxyſm, and that, when this effect is produced, the return of the paroxyſm is greatly retarded, if not entirely prevented.

Dr Currie has alſo found, that the cold bath employed during ſevere paroxyſms of inſanity, has, in ſome inſtances, been productive of very great benefit. But for its ſucceſs in either of theſe diſeaſes, it is neceſſary, that no conſiderable leſion of ſtructure ſhall have taken place, and that the morbid actions be not ſo ſtrong, as to produce inſenſibility to impreſſion; it is alſo neceſſary, he tells us, that the diſeaſes ſhall be ſuch as are ſuſceptible of general paroxyſms; and, finally, that the digeſtion ſhall not have been much impaired, or the vigour of the circulation much debilitated, leſt the action of cold be too ſtrong for the living powers.

To theſe obſervations on the effects of cold water, Dr Currie has added diſquiſitions, on different particulars, in ſome degree connected with his ſubject, eſpecially on the general doctrine of fevers, on the operation of antimonials, opium, and other remedies in fever, on the prevalence of fever among the poor, on the means of preventing fever, on the benefit reſulting from proper hoſpitals in contagious diſeaſes, on animal heat, and on perſpiration; and he concludes the preſent work by an inquiry into the intereſting queſtion, Is there an inhalation by the ſkin?

Both among the ancients and moſt eminent moderns, the opinion that an inhalation took place, was very generally received. The facts and arguments in ſupport of this doctrine, are detailed by the illuſtrious Haller, in the 5th volume of his Elementa Phyſiologiæ. But theſe, Dr Currie thinks, on examination will not be found to afford any direct proof of its truth. It is, he obſerves, controverted by many facts; and, if it ſhould be ultimately eſtabliſhed, it muſt be reared on a new foundation.

In the year 1788, Dr Currie happening to be at Buxton, made an experiment on the effects of bathing on the weight of the body. After half an hour's immerſion in the bath, he found his weight rather diminiſhed than increaſed. In the year 1790, he had a patient in diabetes, whoſe cuticle, as is uſual in that diſeaſe, was in a morbid ſtate. Being deſirous of trying how far the inordinate action of the kidneys might be affected by a gentle ſtimulus applied to the ſkin, he immerſed him in the bath of the temperature of 96 degrees, weighing him before and after the immerſion. In this caſe, no variation in the weight could be detected. Dr Currie afterwards made five different experiments of the ſame kind upon himſelf, varying the heat of the bath from 87 to 95 degrees, but he could never in any inſtance find his weight augmented.

It may, however, be ſaid, that though in diabetes, where the epidermis is diſeaſed, no liquid is inhaled, and though in health, when the veſſels are full, no abſorption may take place, yet that when the body is waſted from a want of the proper food through the ſtomach, the plaſtic power of nature may be employed to ſupply the defect, and to excite an inhalation through thoſe pores on the ſurface, by which an exhalation is uſually performed. To prove that this does not happen, Dr Currie relates very minutely a remarkable caſe of dyſphagia, where death was the conſequence of inanition, notwithſtanding every attempt to ſupport the ſyſtem, both by the rectum and by the ſurface. This patient, on different occaſions, ſtepped perfectly naked upon Merlin's balance, immediately before immerſion; and, again, immediately after it, his body being previouſly dried. The weights were never moved. The reſult was ſurpriſing, for Dr Currie could not diſtinguiſh the ſlighteſt variation in the weight of the body, though the beam would have detected a ſingle dram, though the immerſion had been continued for an hour, and though a conſtant friction had been kept up, nearly the whole of this time, on the inner ſurface of the thighs, with the view of increaſing the action of the abſorbents.

If the non-abſorption by the ſurface of the body be eſtabliſhed, it will, Dr Currie obſerves, aſcertain, that, in the ordinary courſe of things, contagion is received into the ſyſtem by the lungs only, and will, he thinks, juſtify a practice, which, he has been informed, is common among our more experienced ſeamen, on the coaſt of Guinea, and other warm climates, who, when expoſed during the night to a breeze from the marſhes, wrap their heads in a ſea-cloak, or other covering, and ſleep fearleſsly on the deck, with the reſt of their bodies nearly naked.

Notwithſtanding the obſervations of Dr Currie, tending to ſhew, that no inhalation, in ordinary caſes, takes place by the ſurface, which are corroborated by theſe of Mr Seguin, in a valuable Memoir on this ſubject, read before the academy of ſciences of Paris, ſhewing by a variety of ingenious experiments, that while the epidermis is entire, the abſorbents of the ſurface take up neither air nor water, nor any ſubſtance diſſolved in either; but in caſes where a conſiderable portion of epidermis was injured or deſtroyed, as in the itch, the ſpecific effects of a ſolution of oxymuriat of mercury were produced on the ſyſtem; yet the doctrine certainly demands ſtill farther inveſtigation, which we hope our author, or ſome other ingenious experimenter, will beſtow. But whatever determination may ultimately be formed of this particular, we have no heſitation in conſidering Dr Currie as entitled to a high degree of praiſe for this excellent publication.

II.

Deſcriptive Account of a New Method of Treating Old Ulcers of the Legs, By Thomas Baynton, *Surgeon of Briſtol.* 8vo. Briſtol, 1797.

THE common methods of attempting the cure of old ulcers, are, as Mr Baynton juſtly obſerves, tedious, troubleſome, and uncertain. Beſides which, they often become dangerous in their conſequences. It muſt, therefore, be admitted, that any attempts calculated to leſſen the ſufferings of the patient, and the trouble of the ſurgeon, are well entitled to attention.

Mr Baynton aſſerts, that the means which he here propoſes, will, in moſt inſtances, be found ſufficient to accompliſh cures in the worſt caſes, without pain or confinement. Thus, the poor man may be enabled to procure his family-ſuſtenance, while he purſues the means that are calculated to reſtore him to health.

About the commencement of the year 1792, Mr Baynton, after having experienced repeated diſappointments in the cure of old ulcers, for which he had tried every remedy, both external and internal, recommended by the moſt eminent practitioners, determined on endeavouring to bring the edges of theſe ulcers, that might in future be placed under his care, nearer together, by means of ſlips of adheſive plaſter. To this he was chiefly led from having frequently obſerved, that the probability of an ulcer continuing ſound depended much on the ſize of the cicatrix that remained after the cure appeared to be accompliſhed, and from well knowing that the true ſkin was a much more ſubſtantial ſupport and defence, as well as a better covering, than that frail one which is obtained by the aſſiſtance of art. But when he had recourſe to the adheſive plaſters with the view of leſſening the probability of thoſe ulcers breaking out again, he little expected that an application ſo ſimple would prove the eaſieſt, moſt efficacious, and moſt agreeable means of treating ulcers.

Although the firſt caſes in which Mr

Baynton tried this practice were of an unfavourable nature, yet he had ſoon the ſatisfaction to perceive, that it occaſioned very little pain, and materially accelerated the cure, while the ſize of the cicatrices were much leſs than they would have been, had the cures been obtained by any of the common methods.

At firſt, however, his ſucceſs was not quite perfect; as in many inſtances he was not able to remove the ſlips of plaſter, without removing ſome portion of the adjacent ſkin, which, by occaſioning a new wound, proved a diſagreeable circumſtance, in a part ſo diſpoſed to inflame and ulcerate, as the vicinity of an old ſore. He therefore endeavoured to obviate that inconvenience, by keeping the plaſters and bandage well moiſtened with ſpring-water, for ſome time before they were removed from the limb. He had ſoon the ſatisfaction to obſerve, that the inconvenience was not only prevented, but that every ſucceeding caſe juſtified the confidence he now began to place in the remedy. He alſo diſcovered, that moiſtening the bandages was attended with advantages which he did not expect. While the parts were wet and cool, the patients were much more comfortable in their ſenſations, and the ſurrounding inflammation was ſooner removed than he had before obſerved it to be.

By the mode of treatment here recommended, Mr Baynton found, that the diſcharge was leſſened, the offenſive ſmell removed, and the pain abated in a very ſhort time. But beſides theſe advantages, he alſo found, that the callous edges were in a few days levelled with the ſurface of the ſore; that the growth of fungus was prevented, and the neceſſity of applying painful eſcharotics much leſſened, if not entirely done away.

As the ſucceſs of the means here employed very much depends on the mode of their application, he very properly gives a minute deſcription of the method of procedure which he would recommend.

The parts, he tells us, ſhould firſt be cleared of the hair, which is ſometimes found in conſiderable quantities upon the legs; that the dreſſings may be removed with eaſe, at each time of their renewal, which in ſome caſes, where the diſcharges are very profuſe, and the ulcers very irritable, may perhaps be neceſſary twice in twenty-four hours, but which, in almoſt every inſtance, he has been under the neceſſity of performing only once in that ſpace of time.

The plaſter ſhould be prepared for ſpreading, by melting in an iron ladle, over a ſlow fire, four ounces of common, or litharge plaſter, with half a dram of yellow reſin. When melted, it ſhould be ſtirred till it begins to cool, and then ſpread thinly upon ſlips of ſmooth porous callico, of a convenient length and breadth, by ſweeping it quickly from the end that is held by the left hand of the perſon who ſpreads it, to the other end held firmly by another perſon, with the common elaſtic ſpatula that is uſed by apothecaries: The uneven edges muſt then be cut off, and the pieces ſo prepared cut into ſlips of two or three inches in breadth, and of a length that will leave, after being paſſed round the limb, an end of about four inches.

The middle of the piece ſo prepared

ſhould then be applied to the ſound part of the limb, that is oppoſite to the inferior part of the ulcer, ſo that the lower edge of the ſlip may be placed about an inch below the edge of the ſore; and the ends ſhould then be drawn over the ulcer, with as much gradual extenſion as the patient can well bear. Other ſlips ſhould be ſecured in the ſame way, each above the other, until the whole ſurface of the ſore and limb are completely covered with the adheſive ſtraps, at leaſt an inch above and below the diſeaſed part.

The whole of the affected part ſhould then be defended with pieces of ſoft callico, three or four times doubled, and very evenly applied; and a callico-bandage, of about three inches in breadth, and four or five yards in length, or rather as much as will be ſufficient to ſupport the limb, from the foot to the knee, ſhould be applied with as much firmneſs as can be born by the patient, and as much evenneſs as can be obtained by the attention of the ſurgeon, by paſſing it firſt round the leg at the ancle-joint, then once or twice round the foot, and afterwards up the limb, till it reaches the knee; obſerving that each turn of the bandage ſhould have its lower edge ſo placed, as to be about an inch above the lower edge of the fold next below.

The whole of the parts that are at all affected ſhould then be well moiſtened with cold ſpring-water, poured from a large tea-pot; and if the parts be much inflamed, or the diſcharge profuſe, this ſhould be renewed as often as the heat of the parts may indicate, or perhaps at leaſt once in every hour.

The patient may then take what exerciſe he pleaſes; as it will generally be found, that they are eaſier when they walk much, and that their cures are not retarded, but in moſt inſtances accelerated, by their exertion in that way.

Mr Baynton has choſen to apply the means here recommended, at an early hour in the morning, that is, before the œdema has come on, which frequently attends caſes of this kind; firſt, with the view of reſtoring the tone of the refluent veſſels, by ſupporting their ſides when in a natural ſtate;

and, ſecondly, with the expectation of being able to bring the divided edges nearer together, while the parts are all in that ſituation, and the ſkin relaxed, than it would be poſſible to do when the parts are diſtended by tumefaction.

He prefers the uſe of callico to linen, from much experience of its ſuperiority in many reſpects. It does not, he tells us, ſubject the parts to that inconvenient and undue ſtricture, which is experienced from the uſe of linen. It is more pervious, and conſequently prevents the formation of ſinuſes, which might be occaſioned by a complete retention of the diſcharges, if accompanied with the preſſure ſo much recommended. It appears to poſſeſs more of the accommodating properties of the true ſkin, and by its elaſticity it is well calculated to yield a little to muſcular action, while it affords ſufficient ſupport to the parts.

For a bandage, too, as well as for the plaſters, Mr Baynton thinks cotton much preferable either to linen or flannel; it is more elaſtic, ſoft, and yielding, than the former, and, beſides being leſs cumbrous and more cleanly than the latter, it poſſeſſes alſo, he thinks, the additional advantage of being a much better conductor of that morbid heat, which ſo conſtantly affects inflamed parts, and which it is eſſential to remove.

Mr Baynton having thus deſcribed his mode of practice, and explained the principles by which he was directed, next proceeds to give proofs of its ſucceſs, by preſenting us with an account of ſeveral deplorable caſes, which were cured by this means, after the complete failure of ſeveral others. Six caſes are here minutely related, in which this practice appears to have been attended with remarkable ſucceſs. Without entering into a particular detail of theſe, it will here be ſufficient to mention the following concluſions, which Mr Baynton draws from them. The firſt, third, and fourth caſes prove, he thinks, that deep old ulcers ſituated in yielding parts, may, by the means recommended, have their ſides brought into contact, and be cured almoſt as readily by the ſecond intention as recent wounds, with very little remaining ſcar, and conſequently with a diminiſhed probability of relapſe. But he conſiders the ſecond, fifth, and ſixth caſes, as going much farther, and as eſtabliſhing the ſuperiority of this method over every other that has yet been known. For they prove, that ſpeedy cures may be obtained in the worſt and oldeſt ulcers of the pooreſt people, even while the true ſkin cannot be brought forward, ſo as in any degree to cover the denuded parts.

After ſtating theſe concluſions, Mr Baynton propoſes it as a query, To what circumſtances are we to aſcribe theſe remarkable effects? From much ingenious reaſoning, he clearly ſhews, that they cannot be referred to the ingredients of the adheſive plaſter, to the effects of the bandages, to the exerciſe that was uſed, nor to any conſtitutional interference, as the ſame effects have always followed the application of theſe principles, whether the patients have been young or old, robuſt or emaciated, temperate or diſorderly; while ointments compoſed of the ſame ingredients as the adheſive plaſter, bandages, exerciſe, and all the means, except the endeavour to bring the divided parts nearer together, had been tried in every different way without any advantage. But from taking a view of the proceſs of nature, in accompliſhing the cure of divided parts, as deſcribed by Mr John Hunter, and other diſtinguiſhed writers, Mr Baynton thinks it is rendered highly probable, that a great part of the advantages which have been obtained by this practice depend on aſſiſting and imitating nature, by mechanically contracting the granulations, obliterating the extreme veſſels, and overcoming the reſiſtance oppoſed to the proceſs of ſkinning, which are the natural conſequences of the application of the adheſive plaſter, in the way that he has recommended.

III.

Practical Obſervations on the Treatment of Ulcers on the Legs, conſidered as a branch of Military Surgery. By Everard Home, Eſq. F. R. S. *Surgeon to the Army, and to St George's Hoſpital.* 8vo. London, 1797.

WHEN we conſider the number of well-diſciplined men, who are diſcharged from the army on account of ulcers on the legs, it muſt, Mr Home obſerves, appear to be one of the moſt important duties of every military ſurgeon, to pay particular attention to this complaint. In the work now before us, with a view to eſtabliſh ſome general principles, which may guide us in the treatment of ulcers on the legs; the intelligent author has endeavoured to arrange them under different heads, to point out the diſtinguiſhing characters of each ſpecies, and to give an account of the particular effects of thoſe local applications of which he has had experience.

After ſome general obſervations on ulcers, Mr Home proceeds to conſider the circumſtances in which ulcers differ from each other; theſe, in his opinion, are, local peculiarities, conſtitutional peculiarities, peculiar diſpoſitions of the parts or of the conſtitution, and diſeaſed diſpoſitions of the parts or of the conſtitution. He has therefore divided them into ſix different ſpecies.

1. Ulcers of thoſe parts whoſe actions are healthy.

2. Ulcers of thoſe parts whoſe actions are too violent for their powers to ſuſtain.

3. Ulcers where either the parts or the conſtitution have an acquired irritability.

4. Ulcers where either the parts or the conſtitution have an acquired indolence.

5. Ulcers in parts which have acquired ſome ſpecific action, either from a diſeaſed ſtate of the parts, or of the conſtitution.

6. Ulcers where the parts are prevented from healing by a varicoſe ſtate of the ſuperficial veins of the limb.

Of each of theſe ſpecies, he next proceeds to treat ſeparately, ſtating the medicines ſuited to each ſpecies, particularly as applied under the four different forms, of vapour, of liquid, of ointment, or of powder.

He firſt treats of ulcers in thoſe parts whoſe actions are healthy. This ulcer happens when a portion of the ſkin and muſcles is deadened, or even only ſimply divided, if they be not united by the firſt intention. In this ſpecies of ulcer, the pus is white, thick, readily ſeparates from the ulcer, and, when diluted and examined, is found to conſiſt of ſmall globules, ſwimming in a tranſparent fluid. The granulations are ſmall, florid, and pointed at the top. As ſoon as they have riſen to the level of the ſurrounding ſkin, thoſe next the old ſkin become ſmooth, and are covered with a thin ſemitranſparent film, which afterwards become opaque, and forms cuticle.

According to Mr Home, all that is required in the treatment of ſuch an ulcer, is to keep the parts clean, and to prevent their actions from being interrupted in the courſe of the cure. This, he tells us, may in general be beſt done, by applying dry lint to the ſurface of the ulcer, to abſorb the matter, and a pledget of ointment over the whole, to prevent the evaporation of the matter, that the dreſſings may be readily removed at proper intervals. From peculiarities in conſtitution, however, in ſome inſtances, trifling circumſtances will produce diſagreeable effects. With one, a roller moderately tight will make the ulcer loſe its healthy appearance; with another, the application of ointment will diſagree with the neighbouring ſkin, and a third will not heal, unleſs expoſed and allowed to ſcab.

Inquiry, therefore, into ſuch peculiarities, ſhould always be inſtituted; particulary, whether upon former occaſions oily or watery ſubſtances agreed beſt with the ſkin, whether expoſure to air diſpoſed former ulcers to form ſcabs, or the reverſe; and if ſcabs were formed, whether the parts underneath ſkinned over, or not.

Mr Home concludes this ſubject with ſome obſervations on applications conſidered with reference to this ſpecies of ulcer.

1. In the form of vapour, nothing, he tells us, ſhould be applied to this ſpecies of ulcer; ſince the granulations are thereby ren-

dered looſer in their texture, and leſs diſpoſed to form ſkin.

2. In a liquid form, or moiſt ſtate. Poultices are no leſs improper than fomentations; although alcohol is in ſome inſtances found to diſpoſe the granulations to form a ſcab.

3. Applications in the form of ointment, are ſeldom of any other uſe here than to prevent evaporation. And for this purpoſe, the ſimpleſt forms are the beſt, ſuch as that made of equal parts of white wax and olive-oil.

4. In the form of dry powder, any inert ſubſtance, duly applied, will ſoon form a ſcab on the ſurface of the ulcer. But as dry lint will anſwer the ſame purpoſe, Mr Home thinks that, for many reaſons, it is preferable.

A bandage applied tolerably tightly, where there is no peculiarity in the conſtitution to forbid its uſe, gives a firm ſupport to the muſcles and ſkin; and is alſo a defence to the newly formed parts.

Mr Home next treats of ulcers in thoſe parts, whoſe actions are too violent for their powers to ſuſtain. In theſe ulcers, the granulations are larger in ſize than in healthy parts; they are rounded on their external ſurface, leſs compact in their texture, and ſemi-tranſparent. When they arrive at the ſurface, they do not readily form ſkin, but frequently continue to riſe ſtill higher; and then they entirely loſe their diſpoſition to form new ſkin.

For the cure of this ſpecies of ulcer, it is neceſſary, Mr Home obſerves, to pay attention to the conſtitution. Such medicines as ſtrengthen it are to be employed, as bark and ſteel; and whatever is found to affect the conſtitution unfavourably is to be avoided. Wine in ſuch caſes may be given with advantage; but with the claſs of working people, porter is, Mr Home thinks, of much more ſervice. It does not heat ſo much, and they in general like it better.

The firſt object with reſpect to the ulcers themſelves, is to prevent the granulations from riſing higher than the edge of the ſurrounding ſkin. For this purpoſe, according to Mr Home, eſcharotics are not to be employed, but only ſtrong ſtimulant applications,

plications, in conſequence of the action of which the ſuperficial luxuriant parts, to which they are immediately applied, will be abſorbed, and thoſe underneath will be checked in their growth.

It is a great object, Mr Home obſerves, in the healing of an ulcer, to have the new fleſh by which it is to be filled up, as ſtrong in its living powers as poſſible, that the ulcer may not afterwards break out again. For this purpoſe, it is neceſſary to prevent the growth of granulations from being very rapid. And their growth, Mr Home tells us, in an early ſtage of their formation, ought to be kept back by ſuch reſiſtance as they are juſt able to overcome. The proper degree of preſſure may be given by a piece of thin lead over the dreſſings, aſſiſted by a tight bandage on the limb, which, by compreſſing the parts, makes the circulation through the veins leſs tardy than when they are left to themſelves.

In ſpeaking of applications, conſidered with reference to this ſpecies of ulcer, Mr Home obſerves, 1. That fomentations are improper, ſince the application of heat increaſes the actions of parts, gives them a greater tendency to become luxuriant, and renders them ſtill weaker than they were before. 2. That in a watery or moiſt ſtate, poultices are not adapted to this ſpecies of ulcer. The argentum nitratum in weak ſolution in water, the ſtrength proportioned to the ſtate of the ulcer, is one of the beſt applications in a watery form. 3. That in the form of powder, though Peruvian bark, and Lapis calaminaris, have been recommended by ſome; yet Mr Home cannot from his own experience ſpeak in their favour. Powdered carbon, though ſometimes uſeful in theſe, he has found attended with more evident benefit, in ulcers with irritability, than in thoſe with weakneſs. Powdered rhubarb he has found particularly applicable to this ſpecies of ulcer, having a power over the luxuriant growth of granulations, rendering them ſmall and compact, and diſpoſing them to form ſkin. 4. That, in the form of ointment, applications appear to diſagree more frequently with this kind than with any others. In ſome caſes, however, Mr Home has found them to have effect,

fect, where a cure was not to be obtained by other applications. The ointment which he has found moſt applicable to caſes of this kind, is the unguentum hydrargyri nitrati, mixed with hogs-lard, in the proportion of one to five. Common cerate, with a ſmall proportion of the hydrargyrum nitratum rubrum, he alſo recommends as an uſeful application. Both theſe preparations give, he thinks, a check to luxuriant granulations, and are beſt adapted for reducing them, when they have ariſen to too great a height.

Of theſe different medicines, the rhubarb ſeems to Mr Home to agree with the greateſt number of patients, and is therefore the application to which he is diſpoſed to give the firſt trial. But if, after a trial of three or four days, the ulcer appears to be ſtationary, he has next recourſe to a diluted ſolution of the argentum nitratum; and if this alſo does not produce an amendment, he then applies the unguentum hydrargyri nitrati; and, laſtly, he has recourſe to the ceratum epuloticum, or any other common ointment, with the hydrargyrum nitratum rubrum, in the proportion of half a dram to an ounce. Whichever of theſe applications be uſed, he directs that the ulcer ſhould only be dreſſed once in twenty-four hours, unleſs the quantity of diſcharge ſhould prove ſo great as to require it to be done more frequently.

Mr Home next treats of the ulcers where either the parts or the conſtitution have an acquired irritability. In conſidering ulcers of this kind, Mr Home propoſes to treat not only of thoſe which are evidently ſo in their appearance, but to include all ulcers, whatever their appearance may be, which locally or conſtitutionally are ſo irritable, as to require ſedative applications for their cure. What he points out as the principal marks of irritability in ulcers are, the margin of the ſurrounding ſkin being jagged, and terminating in an edge which is ſharp and undermined; the bottom of the ulcer made up of concavities of different ſizes; no diſtinct appearance of granulations, but a whitiſh ſpongy ſubſtance, covered with a thin ichorous diſcharge; and, laſtly, every thing touching the ſurface giving pain, and producing a diſcharge of blood. But ulcers are often of the irritable kind when theſe marks are not preſent; and Mr Home thinks, it is always prudent to take up the treatment under the idea, that the ulcer is irritable.

Medicines in the form of vapour are more immediately applicable to irritable ulcers than to any others. The vapour of common water, Mr Home obſerves, is very ſoothing to many ulcers in an irritable ſtate. The vapour of ſpirits, mixed with that of water, adds to its power of allaying irritation. Opium is uſed with advantage in different forms, ſuch as the tincture ſprinkled on flannels, wrung out of warm water, the extract diſſolved in hot water, or the decoction of poppy-heads. Decoctions of camomile flowers, of the tops of wormwood, or of dried leaves of hemlock, are alſo uſed as fomentations. Although theſe can be applied but for a ſhort time, yet their beneficial effects, Mr Home obſerves, in many inſtances remain for hours after they have been uſed. He adds, however, that there are caſes of the irritable ulcer, which are rendered more painful by the application of any thing warm; and that when this happens, fomentations ought not to be uſed.

In a moiſt ſtate, the moſt ſimple application is the common poultice, made of bread and water, with a little ſweet oil, or of bread and milk. Yet, according to Mr Home, linſeed meal has ſome advantages over bread, as it does not require oil, which ſometimes diſagrees with an ulcer, and as, from having a greater degree of tenacity, it can be kept more immediately on the part, and can be more readily removed, A ſolution of the extract of lead, decoctions of poppy-heads, and carrots, have alſo been with benefit applied under the form of poultice to irritable ulcers.

When poultices agree with an irritable ulcer, the continuance of them will principally depend on the appearance of the granulations. If theſe be ſmall, and the ulcer be rapidly diminiſhing, no change, Mr Home thinks, ſhould be made, till the cure be completed. But if the granulations become larger, and looſer in their texture, he thinks their uſe ſhould be left off, even though the ulcer be evidently contracting.

In a dry form, few medicines are ſufficiently mild to admit of being applied to irritable ulcers. The powder of carbon, however, has lately been highly extolled by ſome practitioners; and Mr Home has found it an uſeful medicine, in many caſes of irritable ulcers, particularly when mixed with the extract of opium.

Applications in an unctuous form are, in general, not well adapted to irritable ulcers. In ſome caſes, however, cream, where it can be procured, is a very ſoothing application. As a ſubſtitute for cream, an ointment made of hogs-lard, purified by frequently waſhing it in ſpring-water, and then made into an ointment, with a ſmall proportion of white-wax and roſe-water, is often uſed with advantage.

In this ſpecies of ulcer, no benefit will be derived from compreſſion. The bandages employed ſhould only be ſufficiently tight to retain the dreſſings in their place.

In the fifth chapter, Mr Home treats of ulcers attended with indolence. In theſe, the appearance is in ſome reſpects the oppoſite of the irritable. The edges of the ſurrounding ſkin are thick, prominent, ſmooth, and rounded. The granulations are ſmooth and gloſſy on the ſurface. The pus is imperfectly formed; it conſiſts of pus and coagulable lymph mixed. The lymph is with difficulty ſeparated from the ſurface of the granulations, and thus gives a white appearance to theſe parts of the ulcer.

But though theſe be the appearances of truly indolent ulcers, yet in ſome caſes they ſo much reſemble thoſe of the irritable kind, that they are to be diſtinguiſhed only by the circumſtance of mild and ſoothing applications being of no ſervice, or even aggravating the ſymptoms.

In indolent ulcers, according to Mr Home, there is a backwardneſs in forming granulations; and in thoſe that are formed, a want of ſufficient ſtrength to bring about a complete cure. Here the great object is to change the diſpoſition of the granulations, and render them ſtrong enough to ſtand their ground after the ulcer is completely filled up.

In enumerating the medicines which he has found beſt adapted to promote the cure of indolent ulcers, Mr Home obſerves, that in the form of vapour, medicines have very commonly been employed in caſes of indolent ulcers; but he does not think that they can promote the cure with advantage.

In a watery form, or moiſt ſtate, many different applications alſo are in uſe. Poultices Mr Home conſiders as applicable only when an irritable ſtate accidentally occurs. In ſuch inſtances, a permanent cure is chiefly to be looked for from the application of proper ſtimulants. As referable to this head, Mr Home conſiders the practice which has been propoſed by Carminati in Italy, and of late ſtrongly recommended by Dr Harneſs, phyſician to the navy, of uſing, as an external application, the gaſtric juice of ruminating animals: It cauſes, according to Dr Harneſs, ulcers in a ſphacelated ſtate to throw off the ſloughs, and put on a milder appearance; and by a continuance of the ſame remedy, a cure may be completed. Mr Home found that this liquid gave a good deal cf pain on each application,

cation, which laſted for near half an hour. From this pain, he is led to conſider it as a ſtimulating remedy; and he has found, that in ſome inſtances the ulcer in a few days put on a better appearance.

In ulcers ſimilar to theſe, in which the ſuccus gaſtricus has chiefly been applied, the freſh root of the caſſada, grated into a pulp, has been ſaid to be employed in the Weſt Indies with evident advantage. Solutions of vitriol and of allum have alſo been recommended. But Mr Home cannot ſpeak in favour of theſe from his own experience.

When an indolent ulcer has no peculiarities, the ſtimulant applications in a watery form, which Mr Home thinks the beſt adapted for a cure, are the following.

1. The ſolution of argentum nitratum is one of the beſt in common uſe. It agrees with indolent ulcers in a greater number of caſes, than almoſt any other medicine. It ſtimulates the granulations, and makes them put on a more healthy appearance; and it has a material advantage, in being capable of having its ſtrength increaſed to any degree that can be required. It is,

however, neceſſary that the ſtrength of the ſolution ſhould be gradually increaſed; for what at firſt gives conſiderable pain, will ſoon ceaſe to have any influence.

2. Tincture of myrrh, either pure or diluted, according to the ſtate of the ulcer, is in many inſtances a good application; it rouſes up the action of the parts, and gives the granulations a better appearance. But when the ulcer is very indolent, it ſoon loſes its effect.

3. Decoction of walnut-tree leaves has been ſtrongly recommended by a ſurgeon of eminence in Vienna, who ſtates, that its powers, in diſpoſing foul ulcers to heal, are beyond thoſe of many other medicines. This application, Mr Home tells us, he has uſed in a number of caſes of indolent ulcers, and can from experience give evidence in its favour.

4. The diluted vitriolic acid has been employed in particular caſes, and recommended as an uſeful medicine in ſuch ulcers. But it has not, Mr Home obſerves, been ſufficiently ſucceſsful to bring it into general uſe.

5. The expreſſed juice of the pod of different ſpecies of pepper, in a recent ſtate, has been uſed as an ingredient in the applications to ulcers of an indolent kind, both in the Eaſt and Weſt Indies. But of theſe Mr Home has no experience.

6. The nitrous acid diluted to ſuch a degree as to fit it for an external application, Mr Home has found to be a very uſeful medicine. No account of the employment of this article in ſuch caſes, has, we believe hitherto been laid before the public. The proportion muſt be varied according to circumſtances, but a ſcruple, to eight ounces of water will in moſt inſtances be ſufficiently ſtrong. Mr Home was firſt led to the uſe of this acid, from reflecting, that it enters the compoſition of the unguentum hydrargyri nitrati, the hydrargyrum nitratum rubrum, and the argentum nitratum, three articles on which his experience led him to place great confidence, and from ſuppoſing, that in conſequence of a decompoſition taking place, the efficient part of the medicine muſt often be the nitrous acid.

On theſe grounds, he was led to try the effects of the acid on ulcers of different kinds. With thoſe that were irritable, it evidently diſagreed. In thoſe attended with weakneſs, in ſome inſtances, and in a very diluted ſtate, it produced an amendment. In others it retarded their progreſs. But in many of the indolent ulcers, he found that it promoted, in a very uncommon manner, the progreſs of the cure. The apparent effect, he obſerves, of the nitrous acid, as an external application, is different from that of moſt other medicines of a ſtimulating nature. It diminiſhes the quantity of matter, and, inſtead of giving a healthy florid appearance to the ſurface of the ulcer, there is a ſoft aſh-coloured coagulated mucus, which partially covers the granulations. On its firſt application, it gives a good deal of pain, which laſts for about an hour, and then goes off. The pain is in general afterwards leſs ſevere. Pus, coagulated by the diluted nitrous acid, forms, according to Mr Home, a better nidus for the granulations than the pus itſelf. But though the coagulation of pus be its principal viſible effect, yet Mr Home is diſpoſed to believe, that it acts upon the granulations as a ſtimulating medicine; too violent indeed for ſeveral ſpecies of ulcers, but adapted peculiarly to ſuch as are indolent.

In the form of powder, the only medicine which ſeems to be adapted to the ulcers attended with indolence is, Mr Home tells us, the hydrargyrum nitratum rubrum, all the others being too mild, or more conveniently uſed in an unctuous form.

Unctuous applications, according to Mr Home, are better adapted to this ſpecies of ulcer than to any other, as they firſt ſtimulate ſufficiently to produce the neceſſary action, and afterwards ſerve as an external covering, by which the pus is confined in immediate contact with the granulations till the next dreſſing. He conſiders the unguentum hydrargyri nitrati, mixed with the adeps ſuilla, in different proportions according to the ſtate of the ulcer, as one of the beſt applications of this kind.

Mr Home concludes his remarks on ulcers attended with indolence, by ſome obſervations on bandages. The great advantages from a ſteady uniform compreſſion in this ſpecies of ulcer, is now, he obſerves, univerſally acknowledged. What he thinks the moſt effectual mode of compreſſion is the laced ſtocking. But a roller of calico or flannel, when neatly applied, anſwers very well; and he conſiders either as preferable to linen, which, from the ſmoothneſs of its ſurface, is apt to ſlip down. Bandages, he tells us, applied partially to the ulcer, do not anſwer, from the difficulty of retaining them upon the part. But this difficulty, he conſiders as in a great meaſure overcome, by the very ingenious and uſeful practice, introduced by Mr Baynton, of obtaining compreſſion by means of ſlips of adheſive plaſter.

After ulcers attended with indolence, Mr Home next treats of thoſe attended with ſome ſpecific diſeaſed action, whether conſtitutional or local. He firſt conſiders the ulcers which yield to mercury. Many ulcers unconnected with the venereal diſeaſe, will heal, under a mercurial courſe, which had received no benefit from other medicines. Theſe ulcers are in general, in appearance, allied to the indolent kind; but have ſome diſeaſed diſpoſition, peculiar to themſelves. Ulcers ſometimes occur on the inſtep and foot, with a very thickened edge, and a diſeaſed ſtate of the ſurrounding ſkin approaching, in appearance to what is called Elephantiaſis. In ſuch caſes, fumigation with the hydrargyrus ſulphuratus ruber, has, Mr Home tells us, healed the ulcer, and removed the ſwelling of the ſurrounding parts.

In ſome caſes, mercurial ointment, mixed with camphor, anſwers better than any other application. The camphor, Mr Home thinks, renders the mercury more active than when employed alone. A dilute ſolution of the hydrargyrus muriatus corroſivus, with a ſmall proportion of ſpirits, is a very uſeful application to many ulcers with a diſeaſed aſpect, more particularly thoſe that are ſuperficial, with a thickened edge, and appear to be principally confined to the ſkin.

Mr Home next ſpeaks of ulcers which yield to different preparations of the conium maculatum. As an external application, he has found the conium maculatum to be a powerful remedy; and he is convinced, that there are diſeaſed ulcers of the legs, which may be cured more readily by the hemlock, than by any other means. In ulcers, near the ankle-joint, with the joint itſelf enlarged, the conium takes off the pain, reduces the ſwelling of the joint, and ſeems to counteract the diſeaſed diſpoſition, whatever be its nature. It is chiefly employed, either in the way of fomentation, or of poultice, made from a ſtrong decoction, either of the dried herb, or freſh leaves. This decoction is often, alſo, advantageouſly applied to ulcers, by means of lint.

In ſpeaking of ulcers which yield to the application of ſalt water, Mr Home obſerves, that ſome of the ſcrophulous kind are more readily cured by this, than by any other application. He has found, that keeping the part immerſed in tepid ſalt-water, for fifteen minutes, twice a-day, is preferable to any other mode of applying it.

In ſome ſuperficial ulcers, attended with a thickening of the ſkin, Mr Home has found the application of tepid ſalt-water produce a cure. Where there is an unuſual coldneſs of the limb, without any tendency to mortification, the tepid ſalt-water, Mr Home tells us, may be uſed with great advantage. And where the leg has a tendency to become anaſarcous, the application of tepid ſalt-water is ſometimes, he obſerves, found to remove that diſpoſition entirely.

There are three different kinds of ulcers, to which a ſolution of the argentum nitratum, applied in different proportions, has more effect than any other application. One of theſe is a leprous eruption, not uncommon among impreſſed ſeamen. Another is the conſequence of buboes, which, from their long continuance, after the venereal virus has been deſtroyed, diſpoſe the ſkin to take on a diſeaſe. And the third is that diſeaſe of warm climates, commonly called, the Ring-worm. In the Eaſt Indies, the application moſt in uſe, for this laſt complaint, is vinegar, ſaturated with borax. The natives employ the juice of ſome plants which are of a very acrid nature, giving the patient excruciating pain, but removing the diſeaſe in a very ſhort time. The ſolution

tion of the argentum nitratum, Mr Home has found to anſwer in mild caſes; and he has little doubt of its being equally ſucceſsful in thoſe that are moſt ſevere.

Mr Home next treats of the ulcers which yield to the uſe of arſenic. Although this article has been more uſed by empyrics, than by regular practitioners, yet it may be employed, Mr Home tells us, in many diſeaſed ulcers with great advantage, both externally and internally. Thoſe ulcers in which he has been chiefly led to employ it, are named from the violence of their diſpoſition, *Noli me tangere*, and are nearly allied to cancer. He here relates different caſes, particularly, one of an ulcer on the tongue, and another on the noſe, in which it was uſed internally with great ſucceſs; and to ulcers of an untoward appearance on the legs, he has uſed arſenic both internally and externally, with great benefit. The ſolution of arſenic, which he has always uſed, is made by boiling white arſenic in water for ſeveral hours in a ſand heat, and taking this ſaturated ſolution for uſe. When given internally, the doſe is from three drops to ten; when applied externally, a dram of the ſolution is diluted with two pounds of water, and the mixture is made gradually ſtronger, till it be of double that ſtrength. This mixture is either applied on lint, or made into a poultice.

The ulcer attended with a varicoſe ſtate of the veins of the limb, is the ſubject of the ſeventh and laſt chapter of this work. This ulcer in its appearance reſembles thoſe of the indolent ſpecies; but if the ſtate of the limb be examined, it will be found that the branches of the ſuperficial veins, which form the vena ſaphena are many of them præternaturally enlarged, and that the vena ſaphena itſelf is extremely large; and it is this enlargement of the veins which prevents the ulcer from being healed.

This ſpecies of ulcer is very common in the army, eſpecially with tall men. The diſeaſe of the veins is, Mr Home believes, frequently brought on by fatigue and change of climate, to which ſoldiers are ſubjected, and by which their general health is ſometimes greatly impaired.

This is the ſpecies of ulcer in which tight bandages on the limb are particularly applicable.

cable. A laced ſtocking is a moſt uſeful application; and if it could be worn without inconvenience, probably no other mode of treatment would be neceſſary. But as the tight bandage and laced ſtocking, the only modes of compreſſing the veins, ſanctioned by general practice, cannot in many inſtances be adopted, and in others are attended with great inconvenience, Mr Home was led to conſider in what other way the ſame effect could be produced. The moſt ready mode of doing this that ſuggeſted itſelf, was that of making an artificial valve, by paſſing a ligature round the vena ſaphena, as it goes over the knee-joint, and obliterating the vein at that part.

This mode of practice Mr Home has now followed for five years, and during that period, he has had opportunities of ſeeing its effects, in twelve different inſtances. Eleven of theſe patients were in St George's Hoſpital, and ſeveral of them ſubmitted to this mode of treatment, from ſeeing the good effects of it in others. The method of performing the operation is very ſimple. As the veins are only turgid in an erect poſture, the operation muſt be performed while the patient is ſtanding; and if he be placed on a table on which there is a chair, the back of the chair will ſerve him to reſt upon, and he will have the knee-joint at a very convenient height for the ſurgeon: The leg to be operated upon, muſt ſtand with the inner ankle facing the light, which will expoſe very advantageouſly the enlarged vena ſaphena, paſſing over the ſide of the knee-joint. While the patient is in this poſture, if a fold of the ſkin, which is very looſe, be pinched up tranſverſely, and kept in that poſition by the finger and thumb of the ſurgeon on the one ſide, and of an aſſiſtant on the other, this fold may be divided by a pointed ſcalpel puſhed through it, with the back of the knife towards the limb, to prevent the vein being wounded, much in the ſame way that the ſkin is divided in making an iſſue.

This diviſion will expoſe the vein ſufficiently, but there is commonly a thin membranous faſcia, confining it in its ſituation; and when that is met with, the vein muſt be laterally diſengaged by the point of the knife, laying hold of the faſcia with a pair of diſſecting forceps, and dividing it. After this, a crooked ſilver needle, with the point rounded off, will readily force its way through the cellular membrane, connected with the vein, without any danger of wounding it, and carry a ligature round it. The patient ſhould now be put to bed, to allow the vein to be in its eaſieſt ſtate, before the ligature be tied, and then a knot is to be made upon the vein. This gives ſome pain, but it is by no means ſevere. The edges of the wound in the ſkin are now to be brought together by ſticking plaſter, except where the ligatures paſs out, and a compreſs and bandage applied, ſo as to keep a moderate degree of preſſure on the vein, both above and below the part included in the ligature. The ligature comes away about the ninth day, ſeldom later than the twelfth, after which the parts commonly heal up. To ſave time, Mr Home has often removed the ligature on the fifth day. The vein lies ſo ſuperficially, that the knot may readily be brought into view, and the point of a pair of ſciſſars paſſed through the ring of the ligature, now become looſe, which may be with eaſe divided, and the whole of the ligature brought away.

As this operation is new, at leaſt for the purpoſe of giving ulcers a diſpoſition to heal, Mr Home has judged it proper, to give a plain narrative of the ſeveral caſes that have come under his care, which may become documents upon this ſubject to others, as they have been to himſelf; and he here details nine caſes, in which the operation was performed in the manner already deſcribed, with a view to the cure of ulcers. In all of them, the ulcers put on a much more healthy appearance, in leſs than three days after the operation. And from that time, where no circumſtances occurred to prevent it, they went on healing like ulcers in healthy perſons. The veins of the limb in all of them became evidently ſmaller next day, in ſome of theſe caſes in a very remarkable degree; and in the courſe of a week they were in general very much diminiſhed in ſize.

Mr Home concludes theſe obſervations by relating four caſes, of varicoſe veins unattended with ulcers, in which the branches became ſmaller, in conſequence of the trunk having been included in a ligature, and rendered impervious at that part. Though theſe caſes be not immediately connected with the treatment of ulcers, yet they furniſh important additional facts reſpecting this operation. They exhibit a comparative view of the conſequences of taking up veins, connected with different parts. Thoſe of the teſticle appear to be more irritable than thoſe of the leg, both in the immediate ſymptoms, and the ſympathetic affections of the conſtitution. The ſymptoms appear evidently to vary in different patients, according to the ſtate of the body at the time; and both in the veins of the teſticle, and of the limb, the preſence of the ligature ſeems to aggravate the ſymptoms very conſiderably, the parts being much relieved immediately after its removal.

We cannot conclude our analyſis of this intereſting work, without obſerving, that the operation which Mr Home has introduced, for the cure of certain ulcers, opens a very wide field for future inquiry and experiment. It is by no means improbable, that this mode of practice may be extended to many other important purpoſes in the practice of phyſic and ſurgery; and that even temporary compreſſion of veins, may in certain circumſtances be followed by beneficial conſequences.

As an appendix, Mr Home has added, that part of his prize-diſſertation of 1788, which treats of the properties of pus, particularly thoſe which diſtinguiſh it from other ſubſtances; the caſes in which it is formed; the time its formation requires; and the effects it has upon the body; as it contains part of the data, from which many of the obſervations of the work itſelf are deduced.

IV.

An Inquiry into the Cauſes and Effects of the Variolæ Vaccinæ, a Diſeaſe diſcovered in ſome of the Weſtern Counties of England, particularly Glouceſterſhire, and known by the name of the Cow-Pox. By Edward Jenner, M. D. F. R. S. &c. *4to*, London, 1798.

THERE is a diſeaſe, by farriers termed the Greaſe, to which the horſe, in his ſtate of domeſtication, is frequently ſubject. It is an inflammation and ſwelling of the heel, from which iſſues matter poſſeſſing properties of a very peculiar kind. This matter, Dr Jenner tells us, after having undergone a certain modification, is capable of generating a diſeaſe in the human body, which bears ſo ſtrong a reſemblance to the ſmall-pox, that he thinks it highly probable, it may have been the ſource of that diſeaſe.

Dr Jenner gives the following account of the origin of this affection. In many places, where a great number of cows are kept, the office of milking is performed indiſcriminately by men and maid-ſervants. One of the former having applied dreſſings to the heels of a horſe affected with the greaſe, and not paying due attention to cleanlineſs, incautiouſly bears his part in milking the cows, with ſome particles of the infectious matter, adhering to his fingers. When this is the caſe, it commonly happens, that a diſeaſe is communicated to the cows, and from the cows to the dairy-maids, which ſpreads through the farm, until moſt of the cattle and domeſtics feel its unpleaſant conſequences. This diſeaſe has obtained the name of the cow-pox.

It appears on the nipples of cows, in the form of irregular puſtules. At their firſt appearance, they are commonly of a paliſh blue, or rather of a colour ſomewhat approaching to livid, and are ſurrounded by an eryſipelatous inflammation. Theſe puſtules, unleſs a timely remedy be applied, frequently degenerate into phagedenic ulcers, which prove extremely troubleſome. The animals become indiſpoſed, and the ſecretion of milk is much leſſened. Inflamed ſpots now begin to appear on different parts of the hands of the domeſtics employed in milking, which quickly run on to ſuppuration, firſt aſſuming the appearance of the ſmall veſications produced by a burn. Moſt commonly they appear about the joints of the fingers, and at their extremities. But whatever parts are affected, if the ſituation will admit, theſe ſuperficial ſuppurations put on a circular form, with their edges more elevated than their centre, and of a colour diſtantly approaching to blue. Abſorption takes place, and tumours appear in each axilla.

The ſyſtem now becomes affected; the pulſe is quickened, and ſhiverings, with general laſſitudes and pains about the loins and limbs, with vomiting, come on. The head is painful, and the patient is now and then even affected with delirium. Theſe ſymptoms varying in their degree of violence, generally continue from one day to three or four, leaving ulcerated ſores about the hands, which, from the ſenſibility of the parts, are very troubleſome, and commonly heal ſlowly, frequently becoming phagedenic, like thoſe from which they ſpring.

The lips, noſtrils, eye-lids, and other parts of the body, are ſometimes affected with ſores; but theſe evidently ariſe from their being needleſsly rubbed or ſcratched with the patient's infected fingers. No eruptions on the ſkin have followed the decline of the feveriſh ſymptoms, in any inſtance that has come under Dr Jenner's inſpection, one only excepted; and in this caſe, a very few appeared upon the arms. They were very minute, of a vivid red colour, and ſoon died away, without advancing to maturation; ſo that Dr Jenner could not determine, whether they had any connection with the preceding ſymptoms.

Thus the diſeaſe, he thinks, makes its progreſs from the horſe, to the nipple of the cow, and from the cow to the human ſubject.

Morbid matter, of various kinds, when abſorbed into the ſyſtem, may, Dr Jenner obſerves, produce effects in ſome degree ſimilar; but what renders the cow-pox virus ſo extremely ſingular, is, that the perſon who has been thus affected, is for ever after ſecure from the infection of ſmall-pox; neither expoſure to the variolous effluvia, nor the inſertion of the matter into the ſkin, producing this diſtemper. In ſupport of this extraordinary fact, Dr Jenner next proceeds to lay before his readers a great number of inſtances.

More than twenty caſes of this diſeaſe are here particularly related, from which we ſhall ſelect only a few of the moſt remarkable. The firſt caſe is that of Joſeph Merret, ſervant to a farmer, who occaſionally aſſiſted in milking his maſter's cows in the year 1770. Several horſes belonging to the farm began to have ſore heels, which Merret frequently attended. The cows ſoon became affected with the cow-pox; and ſoon after, ſeveral ſores appeared on his hands. Swellings and ſtiffneſs in each axilla followed, and he was ſo much indiſpoſed for ſeveral days, as to be incapable of purſuing his ordinary employment. Previouſly to the appearance of the diſtemper among the cows, there was no freſh cow brought into the farm, nor any ſervant employed who was affected with the cow-pox.

In April 1795, a general inoculation taking place in the neighbourhood, Merret was inoculated, with his family, ſo that a period of twenty-five years had elapſed from his having the cow-pox to this time. But although the variolous matter was repeatedly inſerted into his arm, Dr Jenner found it impracticable to infect him with it, an efflorefcence only, taking on an eryſipelatous look about the centre, appearing on the ſkin, near the punctured parts. During the whole time that his family had the ſmall-pox, he remained in the houſe with them, but received no injury from expoſure to the contagion. From the moſt particular inquiries, it was aſcertained, beyond doubt, that he had never had the ſmall-pox.

The fifth caſe related is that of Mrs H. who had the cow-pox when very young. She received the infection in a manner rather uncommon. It was communicated by means of her handling ſome of the ſame utenſils which were in uſe among the ſervants of the family, who had the diſeaſe from milking infected cows. Her hands had many of the cow-pox ſores upon them, and they were communicated to her noſe, which became inflamed, and very much ſwoln. Soon after this event, Mrs H. was expoſed to the contagion of the ſmall-pox, where it was ſcarcely poſſible for her to have eſcaped, had ſhe been ſuſceptible of the diſeaſe, as ſhe regularly attended a relation who had the diſeaſe in ſo violent a degree, that it proved fatal to him. In the year 1778, the ſmall-pox prevailed very much at Berkeley, where ſhe reſided; and as ſhe did not feel perfectly ſatisfied reſpecting her ſafety, Dr Jenner inoculated her with active variolous matter. But although an effloreſcence took place on the arm, no effect was produced on the conſtitution.

Although the cow-pox ſhields the conſtitution from the ſmall-pox, and the ſmall-pox proves a protection againſt its own future poiſon, yet the human body is again and again ſuſceptible of the infectious matter of the cow-pox. This is demonſtrated by the ninth caſe which Dr Jenner relates, viz. that of William Smith of Pyrton, in the pariſh of Berkeley. He contracted the diſeaſe in the year 1780. It fell to his lot to attend a horſe with ſore heels. By this means, the infection was carried to the cows, and from the cows it was communicated to Smith. He was affected with the common ſymptoms of the diſeaſe, and on one of his hands were ſeveral ulcerated ſores. In 1791, the cow-pox broke out at another farm, where he then lived as a ſervant; and he became affected with it a ſecond time. In the year 1794, he was ſo unfortunate as to catch it again. The diſeaſe was as ſevere the ſecond and third time as the firſt. Dr Jenner, however, obſerves, that a ſecond attack is commonly very ſlight, both with the human ſpecies, and likewiſe among cows. In ſpring 1795, Smith was twice inoculated, with variolous matter, but no affection of the ſyſtem could be produced; and he has ſince aſſociated with thoſe who had the ſmall-pox in its moſt contagious ſtate, without feeling any effect from it.

The more accurately to obſerve the progreſs of the infection in the cow-pox, Dr Jenner communicated it to many different perſons, by inoculation; as an example of which, we ſhall here give an account of the 17th caſe. Matter taken from the arm of a dairy-maid, infected by her maſter's cows, was inſerted on the 14th of May, 1796, into the arm of a healthy boy, about eight years old, by means of two ſuperficial inciſions, barely penetrating the cutis, each about half an inch long.

On the ſeventh day, he complained of uneaſineſs in the axilla; and on the ninth, he became a little chilly, loſt his appetite, and had a ſlight headach. During the whole of this day, he was perceptibly indiſpoſed, and ſpent the night with ſome degree of reſtleſſneſs. But on the day following, he was perfectly well.

The appearance of the inciſions, in their progreſs to a ſtate of maturation, were much the ſame as when produced, in a ſimilar manner, by variolous matter. The only difference which Dr Jenner perceived, was in the ſtate of the limpid fluid, ariſing from the action of the virus, which aſſumed rather a darker hue, and in that of the eﬄoreſcence ſpreading round the inciſions, which had more of an eryſipelatous look than is commonly perceived, when variolous matter has been uſed in the ſame manner. But the whole died away, leaving on the inoculated parts, ſcabs, and ſubſequent eſchars, without giving the leaſt trouble.

After this ſlight affection of the ſyſtem, from the cow-pox virus, this boy was inoculated, on the 1ſt of July 1796, with variolous matter, immediately taken from a puſtule. Several ſlight punctures were made on both arms, and the matter was carefully inſerted, but no diſeaſe followed. Several months afterwards, he was again inoculated with variolous matter, but no ſenſible effect was produced on the conſtitution.

In other inſtances of inoculation with the matter of cow-pox, which are here related, Dr Jenner had proof, that the matter, in paſſing from one human ſubject to another, through five different gradations, loſt none of its original properties.

After relating the different caſes which eſtabliſh this point, Dr Jenner concludes his inquiry, with ſome general obſervations on the cow-pox. Although he imagines, that farther teſtimony is unneceſſary to ſupport his aſſertion, that the cow-pox protects the human conſtitution from the infection of the ſmall-pox, yet it affords him ſatisfaction, that this point has been confirmed by the concurring teſtimony of Mr Dolland, a ſurgeon, who reſides in a dairy country, remote from that in which his experiments were made.

The active quality of the virus, from the horſes heels, is, Dr Jenner obſerves, greatly increaſed after it has acted on the nipples of the cow, as it rarely happens, that the horſe affects his dreſſer with ſores, and as rarely that a milk-maid eſcapes infection, when ſhe milks infected cows. It is alſo, he remarks, curious to obſerve, that the virus, which, with reſpect to its effects, is undetermined, and uncertain, previouſly to its paſſing from the horſe, through the medium of the cow, ſhould then, not only become more active, but ſhould completely poſſeſs thoſe ſpecific properties, which induce, in the human conſtitution, ſymptoms ſimilar to thoſe of the variolous fever, and effect in it, that peculiar change which renders it unſuſceptible of the variolous contagion. May it not, he aſks, be reaſonably conjectured, that the ſource of the ſmall-pox is a morbid matter, of a peculiar kind, generated by a diſeaſe in the horſe, and that accidental circumſtances may have again and again ariſen, ſtill working new changes upon it, until it has acquired the contagious and malignant form under which we now commonly ſee it, making its devaſtations among us?

As fatal effects have never been known to ariſe from the cow-pox, and as it clearly appears, that this diſeaſe leaves the conſtitution in a ſtate of perfect ſecurity from the infection of ſmall-pox, may we not, ſays Dr Jenner, infer, that a mode of inoculation may be introduced, preferable to that at preſent adopted, eſpecially among thoſe families which, from previous circumſtances, we may judge to be prediſpoſed to have the diſeaſe unfavourably. It is an exceſs in the number of puſtules which we dread in the ſmall-pox. But in the cow-pox no puſtules appear; nor does it ſeem poſſible to produce the diſeaſe from effluvia, or by any other means than contact, and that probably not ſimply between the virus and the cuticle; ſo that a ſingle individual in a family might at any time receive it without the riſk of infecting the reſt, or of ſpreading a diſtemper which fills a country with terror.

In ſome other points of view, the inoculation of the cow-pox, appears to Dr Jenner preferable to the variolous inoculation. In conſtitutions prediſpoſed to ſcrophula, the inoculated ſmall-pox, even when very mild, often rouſes into activity that diſtreſsful malady; a conſequence which has not been obſerved from the cow-pox. Some, from peculiarity of habit, reſiſt variolous inoculation, and are by this means conſtantly haunted with the diſtreſſing idea of being inſecure from ſubſequent infection. The inoculation of the cow-pox affords, Dr Jenner obſerves, a ready mode of diſſipating anxiety from ſuch a cauſe. And beſides this, as the conſtitution may at any time be made to feel the febrile attack of cow-pox, he conjectures, that it might in many chronic diſeaſes be introduced into the ſyſtem with the probability of affording relief. Theſe, however, and other ſpeculations, muſt be confirmed or refuted by future obſervation; and it is to be hoped, that what is here preſented to the public, by Dr Jenner, will direct to this ſubject the attention of others, who practiſe medicine in parts of the country where the diſeaſe appears. Dr Jenner concludes with obſerving, that he ſhall himſelf continue to proſecute his inquiries on this ſubject, encouraged by the hope of its becoming eſſentially beneficial to mankind.

The appearance of the eruptions in its various ſtages, both as produced by accidental infection and by inoculation, is illuſtrated by four well engraved plates, coloured after nature.

V.

Eſſays on the Venereal Diſeaſe, and its Concomitant Affections. Illuſtrated by a variety of Caſes.—Eſſay I. *Part* I. *On the Antivenereal Effects of the Nitrous Acid, Oxygenated Muriate of Potaſh, and ſeveral analogous remedies, which have been lately propoſed as ſubſtitutes for Mercury. By* William Blair, A. M. *Surgeon of the Lock Hoſpital and Aſylum, and of the Old Finſbury Diſpenſary.* 8vo. London, 1798.

THE uſe of the nitrous acid for the cure of the venereal complaints, has of late attracted more notice than any other new practice. While ſome men of diſtinguiſhed eminence are ſtrenuous advocates in ſupport of it, others, of no leſs high reputation, have no ſcruple in repreſenting it as inefficacious, and even as pernicious. To the evidence on this ſubject, already offered by others, Mr Blair, who, from his ſituation, has many opportunities of practiſing in venereal complaints with peculiar advantages, has here added thoſe teſtimonies which his experience affords.

After ſome preliminary obſervations, Mr Blair gives us, in the ſecond chapter, a ſummary view of the atteſtations, which have hitherto been given of the efficacy of the new antivenereal remedies, particularly thoſe of Mr Scott, Dr Currie, Dr Rollo, Dr Beddoes, &c.; and in the third chapter, he preſents us with unfavourable reports, from the teſtimonies of Mr Bowles, Dr Girdleſtone and Mr Bell. Of theſe, however, an account has already been given in our former volumes. We ſhall therefore now content ourſelves with preſenting to our reader, a ſhort analyſis of the fourth chapter, in which we have the author's own experience of the effects of the new antivenereal remedies.

In the firſt ſection of this chapter, twenty-three experiments, with the acid of nitre, the citric acid, and the oxygenated muriat of potaſh, againſt primary ſymptoms, are related. It will here be ſufficient for our purpoſe,

poſe, to ſelect a few of theſe caſes. The firſt caſe, in which Mr Blair employed the nitric acid, ſeems to afford evidence in ſupport of its efficacy. George Kelly, aged thirty, was admitted into the Lock Hoſpital, on the 30th of March 1797, with a glandular ſwelling in his right groin, which ſeemed inclined to ſuppurate. He contracted the venereal diſeaſe two months before, and at that time had chancres. But when he applied to Mr Blair, they were healed. His mouth was alſo affected with mercurial medicines, which he had previouſly taken.

On the 4th of April, Mr Blair preſcribed for him, three meaſured drams of the common fuming nitrous acid, in a concentrated ſtate, diluted with a quart of water. This quantity was ordered to be taken every twenty-four hours, by drinking a tea-cupful at a time. On the 6th of April, the acid was increaſed to four drams, as it agreed perfectly well. His gums, which before were rather ſwelled and tender, had now returned to a natural ſtate, and the ſize of his bubo was a little reduced.

On the 10th, the tumour had nearly diſappeared.

appeared. The acid was continued in the ſame proportion. He complained of a tenderneſs in his cheeks, which probably aroſe from the local effects of the acid, but he had no inclination to ptyaliſm. On the 13th, his mouth was in the ſame ſtate, and the bubo quite gone. On the 17th, as the acid now gave him violent pains in his bowels, a grain of opium was directed to be taken occaſionally; but it was continued without interruption till the 24th; and on the 27th, he was diſmiſſed, apparently cured. As Mr Blair has not ſince heard of his having been ſubjected to any return of his complaints, we may conclude, that in this inſtance the venereal poiſon was, with ſafety and eaſe, overcome by the nitrous acid.

The ſecond caſe was by no means equally in favour of the acid. A patient in the eighteenth year of his age, who had laboured under venereal ſymptoms for three months, was admitted into the Lock Hoſpital, on the 16th of September 1797. For three days he rubbed in a dram of mercurial ointment daily; but Mr Blair afterwards directed him, to take daily two drams of the nitric acid, mixed with a pint and a half of water, and four ounces of ſimple ſyrup. This medicine was begun on the 21ſt of September; and agreed well with him. His ſymptoms, particularly a bubo in his groin, ſoon began to diminiſh. By the 16th of October, the bubo was diſperſed. By the 25th, he appeared to be quite well; and on the 2d of November, till which time the medicine was regularly continued, he was diſmiſſed, apparently cured. During this period, he took, Mr Blair obſerves, 84 drams of the acid by meaſure, which, upon calculation, was found to be equal to 147 drams by weight, which he conſidered as ſufficient for giving the patient a fair trial for a perfect cure. But, notwithſtanding the total diſappearance of the ſymptoms, during the uſe of the acid, he returned to the Hoſpital in twenty-eight days after he was diſmiſſed, having numerous venereal ſpots upon his face and body, with nocturnal pains in his limbs, and an ophthalmia tarſi. Mr Blair, conſidering the eruption as deciſive of a venereal affection, ordered him mercurial inunction, and, after a regular courſe, he was diſmiſſed on the 28th of January following, in perfect health. Mr Blair did not obſerve, that in this inſtance the cure was effected by leſs mercury, or in a ſhorter time, than is uſual in ſecondary affections. But although the nitric acid did not eradicate the diſeaſe, as is but too often the caſe with a mercurial courſe, improperly employed, yet, from the apparent removal of the ſymptoms after they had continued for a conſiderable time, there is ſtrong reaſon for preſuming that it counteracted the morbid poiſon, at laſt to a certain degree. In ſome other caſes ſimilar apparent benefit reſulted from the acid, but in almoſt all of them, it was neceſſary afterwards to have recouſe to mercury. And in ſome inſtances, the acid ſeemed to have no effect whatever, as in the 24th caſe, the laſt which is related in this ſection, viz. that of Richard Purſall. He applied to Mr Blair, at the Finſbury Diſpenſary, on the 10th of November 1797, with an indurated ſwelling in one of his groins, occaſioned by cohabitation with an infected woman. The nitrous acid was preſcribed for him, which he took, after being properly diluted to the extent of from two to three drams every day, until the 1ſt of January. During this time the tumour inflamed, ſuppurated, and broke ſpontaneouſly. It continued ſome weeks in an ulcerated condition, and did not ſeem at all to granulate. Mr Blair then adviſed mercurial friction, without changing the dreſſings previouſly employed. The ſore was nearly healed in eight days, under this medicine; and on the 24th of January he was diſcharged, well.

The ſecond ſection of this chapter contains twenty-ſix caſes of confirmed ſyphilis, in which the acid of nitre was exhibited. In theſe it was found incapable of eradicating the diſeaſe, although in ſome of them it was taken to the extent of upwards of two hundred drams; and in ſome recourſe was had to nitric acid, carefully prepared by Mr Seaton, with all the requiſites for accurate experiments, being oxydated, and freed from its ſuperabundant azote, by diſtillation from mangaueſe. It was, however, found that this acid was no more to be depended on, for curing ſyphilis than the common fuming nitrous acid. Although, in ſome inſtances, temporary good effects were produced, yet, from the caſes here related, Mr Blair infers, that no one could heſitate a moment in giving the preference to mercury.

In the third and laſt ſection of this chapter, which concludes the Part of Blair's Eſſays now publiſhed, we are preſented with an account of eleven trials, made with the oxygenated muriat of potaſh, in advanced ſtages of Lues Venerea. But with this medicine alſo, though it has been recommended as far ſuperior to the acids, Mr Blair was not more ſucceſsful. In the firſt caſe, the oxygenated muriat of potaſh employed, was procured from Mr Coxwell, near Temple-bar, and ſuppoſed to be of the beſt quality. The patient beginning with five grains on the 12th of october, had the doſes gradually increaſed till he took ſeventy grains daily. But when uſed to this extent, the medicine occaſioned a violent pain in his ſtomach, bowels, and head; and was ſo obnoxious to his feelings, that on the 6th of November he diſcontinued taking it. At this time he had taken two ounces and a half of oxygenated muriat of potaſh. By an overſight it happened, that ulcerated blotches, and incruſtations on different parts of his body, had been dreſſed with a cerate in which red nitrated mercury had been mixed. Some of the ſores were healed by this dreſſing, but others had become deeper and more virulent, eſpecially thoſe on his face. The ſmaller eruptions were not altered in their appearance; an ophthalmia, which afterwards deſtroyed one eye, attacked him on the 2d of November; a ſalivation enſued, amounting to a pint and a half or two pints in twenty-four hours; his gums became a little tender, his appetite entirely failed him, and he had at laſt a white tongue, frequent and ſoft pulſe, with extreme proſtration of ſtrength. Death was now looked for, as the only relief to his ſufferings. Mr Blair, however, put him on a mercurial courſe, though with hardly a ray of hope for his recovery. He fumigated his throat twice a-day with cinnabar, and rubbed in the ointment by ſlow degrees. He ſhewed ſigns of amendment in ſix days, and in a ſhort time was aſtoniſhingly recovered. But unhappily a bad diarrhœa coming on, reduced him to his former ſtate of weakneſs. His venereal ſymptoms, however, continued to get ſurpriſingly better. But his relations removed him from London, and Mr Blair had not afterwards an opportunity of hearing of him.

In the other caſes which Mr Blair has here related, the oxygenated muriat of potaſh, was procured at Mr Speed's of Cannon Street, who had furniſhed what was conſtantly uſed with ſucceſs by Dr Rollo at Woolwich. But as employed by Mr Blair, it was not productive of any material benefit; in farther proof of which, it will here be ſufficient to preſent to our readers, a ſhort account of the laſt caſe.

Daniel Duncarty, aged forty, became infected with the venereal diſeaſe about Michaelmas 1796. He was long neglectful of his ſituation, and never took any mercury. On the 28th of September 1797, he was admitted at the Lock Hoſpital, with very numerous eruptions of five weeks duration. On the 2d of October, he began to take two drams of the nitric acid from Apothecaries-hall, properly diluted and ſweetened.

It agreed with him, and the eruptions ſoon began to fade. Till the 16th of October, the warm bath had been uſed twice a-week, together with the nitric acid. He was now ordered to deſiſt from it, and to continue the acid alone. On the 19th, he ſtill continued to mend, though his lips and cheeks were now ulcerated by the local action of the acid. A conſiderable ſpitting ſoon ſupervened; and in the beginning of November, the daily proportion of acid was reduced to one dram, on account of the ſtate of his mouth. The ſpots were now as much diſperſed, as they would have been by a mild mercurial courſe. But a ſmall glandular ſwelling had appeared above his left elbow, which gave conſiderable pain, and an hoarſeneſs, peculiar to the venereal diſeaſe, had likewiſe begun to be troubleſome.

The nitric acid was continued till the 13th; and as he had uſed it regularly for forty days, it was deemed ſuperfluous to perſiſt any longer. From that period till the 4th of January, he was abſent from the hoſpital, and took nothing. He then returned with his face as thickly beſet with puſtular ſyphilitic eruptions as Mr Blair had ever ſeen, and his hoarſeneſs was alſo very bad. As a fair trial had been given to the acid, Mr Blair thought of giving as full a one to the oxygenated muriat of potaſh. This was accordingly begun on the 5th of January 1798, to the extent of thirty grains daily, and the medicine was regularly continued till the 8th of February, when he was taking two hundred grains each day, without any inconvenience to his ſtomach. The ſpots, however, were become more numerous, though he had taken upwards of ſeven ounces of the oxygenated muriat of potaſh. Mr Blair, therefore, conſidered it frivolous, to perſevere longer; and he was put upon a courſe of mercurial inunction, which he bore well. The venereal ſymptoms, however, did not yield more readily than uſual, and he eloped unexpectedly from the hoſpital, before he was completely cured. Mr Blair's experience, therefore, affords ſtrong proof of the inefficacy of the oxygenated muriat of potaſh.

VI.

VI.

Verſuche ueber die gereitzte Muſkel-und Nervenfaſer, nebſt Vermuthungen ueber den chemiſchen Proceſs des Lebens in der Thier- und Pflantzenwelt.

Experiments on Stimulated Muſcular and Nervous Fibres; with Conjectures on the Chemical Proceſs of Animal and Vegetable Life. By Fred. Alex. Von Humboldt. *Vol.* 1. *with plates,* 8*vo.* Poſen and Berlin, 1797.

THE number of new facts, and the ingenuity of the opinions, contained in this work, will ſufficiently apologize for the length to which its analyſis may extend. We are rather afraid, that, by endeavouring to bring it within the limits proper for this publication, it may have loſt much of its perſpicuity and intereſt.

In the preface, Mr Humboldt informs us, that, for ſeveral years, he has been comparing the phenomena of animal ſubſtances with the laws of dead matter. During this inquiry, he has ſucceeded in experiments, which, he thinks, throw conſiderable light on the chemical proceſs of life. A ſeparated animal organ, furniſhed with irritable and ſenſible fibres, can, in a few minutes, have its excitability rouſed from the loweſt to the higheſt degree, and again depreſſed, at pleaſure. He has operated upon animal organs, for hours together, with oxymuriatic acid, alkalies, nitric acid, oxyd of arſenic, opium, and alcohol; and they have eſcaped from the war of contending elements, almoſt unhurt. He has found, that animal bodies have the power of acting at a diſtance; and has endeavoured to demonſtrate to the ſenſes, its limits, which decreaſe as the vital power decreaſes. He thinks he is able to prove, that irritability does not depend on the quantity of oxygen alone; but that azot and hydrogen are of much greater importance; and that it conſiſts in the mutual action and oppoſition of different principles.

He commences with an account of his experiments on Galvaniſm; not becauſe they are the moſt important part of his work, but becauſe they led him to the ſubſequent reflections. And as many of them directly contradict the uſually received opinions, he anticipates any ſuſpicion of their inaccuracy, by aſſerting, that there is not one important experiment detailed, which has not been repeated for hours, on eight or ten different individuals, on well dried plates of glaſs, before ſeveral competent witneſſes, who carefully examined every circumſtance.

To avoid circumlocution, and connection with any theory, he has adopted the terms of galvanic ſtimulus, for the external unknown cauſe of the muſcular motions; to galvaniſe, for its application; and galvaniſm, for the aggregate of all the phenomena connected with it. Galvaniſm is only obvious where there are ſenſible fibres; it preſuppoſes the re-action of animal vital power, while the production of muſcular contractions, by means of animal homogenous matter alone, ſeems to ſhew that the nervous fibre is not paſſively affected, during the irritation; but that the whole agency is in itſelf. Although the galvanic experiment ſucceeds only with excitable matter, it may have collateral effects on inanimate ſubſtances connected with it. Hitherto, however, theſe have not been rendered obvious. Our author maintained, for a conſiderable time, the probable flow of the ſuppoſed galvanic fluid, through coloured liquids, and ſaturated ſolutions of ſalts, without being able to obſerve any effect whatever. In imitation of Chaldni's experiments on ſound, he likewiſe ſtrewed plates of zinc and ſilver, with finely powdered marble, and ſeeds of the lycopodium; but galvanization produced none of the chaldnic figures. The ſtrongeſt batteries of iſchiadic and crural nerves, neither affected Bennet's electrometer, nor the hair left on the galvaniſed thighs of mice.

If the principal agent in galvaniſm be the application of a ſtimulus, the ſucceſs of the experiment muſt depend equally on the ſtrength of that ſtimulus, and the excitability of the muſcular organ. Hitherto, the firſt condition has alone been attended to; and it was reſerved for our author to notice the laſt. With torpid animals, the ſtimulus may exiſt without exciting contractions; and it is therefore of the utmoſt importance, in determining the laws of galvaniſm, to operate on lively and irritable individuals. Our author hence employed young, ſtrong, and (as often as poſſible) female frogs. He found them moſt irritable in cool ſummer-weather, or after being awaked out of their long winter-ſleep, and fed ſeveral together in a room for ſome days. But his diſcovery of the means of artificially increaſing excitability, by the application of alcaline ſolutions, or oxymuriatic acid, to the nerves, is one of the moſt important he has made. He was able to raiſe the excitability of torpid animals, to a degree to which he met with none analogous in the natural ſtate; and, by the exceſſive application of ſtimuli, or of common acids, it was eaſy to lower or extinguiſh it. Our author now proceeds to determine the conditions under which the galvanic phenomena appear; and, for conſidering them better individually, he aſſumes two diſtinct ſtates of animal matter; that of naturally great, or artificially increaſed excitability; and that of diminiſhed excitability.

In the ſecond ſection, Mr Humboldt clearly proves, that galvanic contractions may be excited, without the intervention of metals, or of charcoal, when freſh animal matter conſtitutes the conductor between the nerve and muſcle. This was firſt obſerved by Cotugno of Naples. Galvani afterwards produced them, by ſkinning and emboweling a frog, and cutting through all connection between the lower and upper extremities, except the iſchiadic nerves, as often as he brought the muſcles of the leg in contact with the ſhoulder. Volta, at firſt, aſcribed the contractions in this caſe to mechanical compreſſion; but, afterwards, found it conſonant with his theory; imagining in the nervous, muſcular, and tendinous parts, the three heterogeneous matters, whoſe mutual contact he thinks neceſſary to determine the electricity to flow in one or other direction. But our author excited contractions, by bringing the red muſcles of the loin in contact with the iſchiadic nerve. Here were only two organically connected matters; nor could the effect be poſſibly owing to mechanical preſſure, as it was not produced by the application of wax, wood, &c. Volta himſelf placed the upper and lower extremities of a frog, in two different glaſſes of water; and cauſed contractions by connecting them with his fingers. Our author cut off both legs of a lively frog; and having prepared its nerve quickly, he laid the right extremity on a dry plate of glaſs. He now faſtened four or ſix cubic inches of freſh muſcular fleſh to an inſulating handle of ſealing wax, and with it touched at once the nerve and muſcles of the leg. Contractions enſued. With dry wood, horn, and other non-conducting ſubſtances, the experiment did not ſucceed. Nay, farther, he touched both nerve and muſcle with two pieces of fleſh, fixed to different inſulating handles; and no contractions followed, until they were made at the ſame time to touch each other, or were connected by a third piece. The left leg, which had lain untouched more than fifteen minutes, and was almoſt inſenſible to mechanical preſſure, being now prepared, exhibited the ſame phenomena; during which, Mr Humboldt obſerved, that the irritation was much greater, when the fleſhy conductor firſt touched the muſcle, and then the nerve, than when this order was reverſed. Theſe experiments ſucceeded with nerves whoſe excitability was too ſmall in their natural ſtate, as ſoon as they were moiſtened with an alkaline ſolution. In ſeveral inſtances, he cut off the upper part of the crural nerve, and on bringing it into contact with the remaining part, and with the muſcle, by means of a glaſs tube, contractions followed. A more ſimple experiment cannot well be deviſed, as the only agents were nerve and muſcle. It is, however, remarkable, that when the nerve, or connecting bit of fleſh, is not ſufficiently freſh to produce contractions, when moved by a glaſs rod, they will ſucceed, if, in its place, an iron wire be employed, though it form no part of the chain. Mr Humboldt alſo ſucceeded ſeveral times, when he made the arch between two parts of the nerve, by holding it gently with one hand, and touching it with a piece of fleſh held in the other. To avoid all ſuſpicion of mechanical preſſure, he ſimply touched the nerve in two points, with pieces of fleſh held in each hand. The contractions were moſt violent, when with the one he touched the nerve near its inſertion into the muſcle. The galvanic experiment, therefore, ſucceeded with Mr Humboldt,

1ſt, When he bent back the loin of an animal, ſo as to touch the iſchiadic nerve, with which it was ſtill organnically connected.

2dly, When the crural nerve and muſcle of the leg were touched at the ſame time, by a ſeparated piece of the crural nerve.

3dly, When animal matter formed the arch from one part of the nerve to another.

His experiments, on the contrary, proved negative, that is, were not attended with any obvious effect, when he bent back the leg againſt its own nerve, ſtill organically connected with it, and when, without touching the muſcle, he brought a piece of ſeparated nerve in contact with the crural nerve in two points.

The ſecond condition, under which, in the ſtate of great excitability, contractions occur, ſuppoſes the co-operation of ſubſtances, containing metals or carbon; both of which, for the ſake of brevity, he includes in the term Metallic. This may be ſubdivided into two caſes; irritation by means of one metal, or homogeneous metallic bodies; and irritation, by means of heterogeneous metals. When homogeneous metals cauſe contractions, they are applied, either ſo as to form a continued chain with the animal organs, through which a fluid might circulate; or they do not form it. The laſt caſe is contrary to the univerſally-received opinion; but, in ſeveral very accurate and ingenious experiments, Mr Humboldt produced irritation, by touching the metal, upon which the nerve reſted, with another metal, either homogeneous or heterogeneous. The reverſed experiment, that is, when one piece of metal touched the nerve, and another the muſcle only, without conſtituting an arch, was always negative.—The nerves of two legs, between which there was no conducting connection, were armed with the ſame piece of zinc; and, at firſt, both contracted violently, when the zinc was touched with another piece of the ſame metal. At laſt, the one leg became exhauſted; but contractions were renewed in it as ſoon as it was connected with the other, either by a piece of zinc, or by their being brought into contact with each other.

No diſpute, relative to galvaniſm, has been carried on more keenly in Italy, than, Whether homogeneous metals be capable of producing contractions or not? And a multitude of inaccurate and inconcluſive experiments have been made with the metals of commerce, in order to decide it. Galvani prepared a frog, as uſual, ſo that the rump and leg were connected by the iſchiadic nerve only. Each extremity lay in its own glaſs of water; and, on connecting them by a metallic arch, they were convulſed. Volta repeated his experiments before our author. A ſilver arch, connecting the water in the glaſſes, produced no contractions. A particle of ſoap, potaſh, or acid, rubbed on the one end of the arch, inſtantly excited them. Both ends were immerſed in the ripe juice of the cornus maſcula. No contractions followed. One end was wiped and moiſtened with the unripe juice of the ſame fruit, and they took place. Hence, Mr Humboldt agrees with him, that animals in which homogeneous metals excite no contractions, are convulſed, when the metals are rendered unlike by the ſmalleſt change in their compoſition, poliſh, hardneſs, form, or temperature; but by no means allows, that, only when the metals are unlike, muſcular contractions are produced.—Aldini of Bologna's ingenious experiment with quick-ſilver, did not ſatisfy Volta. Our author, therefore, varied it, by purifying ſome quick-ſilver to the higheſt degree poſſible, and ſuſpending over it, the prepared leg of a frog, ſo that the nerve and a piece of muſcle hung downwards. The nerve was then gently lowered, ſo as to touch the ſurface of the mercury, and no contractions followed; but when the muſcle alſo came in contact with the metallic ſurface, they took place. Two ſimilar bits of muſcular fleſh were then made to float in the liquid metal. When the contact now began with the nerve, the ſtimulus was too weak to cauſe any motion; but when the muſcle was firſt lowered, and afterwards the nerve, excitement followed. If, in place of muſcular fleſh, bits of paper were employed, the experiment was always negative. Mr Humboldt alſo relates ſome leſs ſimple experiments, in which two pieces of zinc produced contractions, when gold and ſilver had failed.

When heterogeneous metals, or ſubſtances containing carbon, are intended to produce contractions in a ſtate of great excitability, it is ſufficient, that in any part of the chain of conducting ſubſtances two diſſimilar exciters be found.

Contractions take place when heterogeneous metals coat the nerve and muſcle; and are either brought into immediate contact, or are connected by means of a moiſt conducting ſubſtance.

When homogeneous metallic coatings are connected by a heterogeneous metal.

When homogeneous metallic coatings are each connected with a heterogeneous metal, by moiſt conductors.

When, in a chain between the nerve and muſcle, compoſed of ſeveral metals, and alternate pieces of fleſh, one only is homogeneous.

When, in all the above caſes, the nerve and muſcle are only connected with their coatings by means of moiſt conducting ſubſtances.

Theſe five principal caſes may be varied infinitely; and ſome of them require a higher degree of excitability, to be ſucceſsful, than others.

When the excitability of the organ is ſo ſmall, that neither ſimple animal ſubſtances, homogeneous metals, nor heterogeneous, not forming a chain, produce any effect, galvanization may ſtill be rendered obvious, when heterogeneous coatings of the nerve and muſcle are either brought into immediate contact, or are connected by exciting links; or when homogeneous coatings are connected by a heterogeneous metal, one ſurface of which is moiſtened by an evaporating fluid.

In this latter caſe, (which our author diſcovered by accidentally breathing upon an inactive chain of metals, which immediately produced contractions), he found that the ſtimulus was more active, in proportion as the fluid evaporated more quickly. Thus, ether was more active than warm water; warm water, than cold. If upon the zinc coating of a torpid nerve a drop of water was placed, contractions were excited by touching it with the muſcular coating; though the contact of the two dry metals produced no effect. In place of a drop of water, he laid a piece of freſh muſcular fleſh on the nervous coating; and found, to his aſtoniſhment, that, on approaching the muſcular coating within a Paris line of it, contractions enſued; if a thin plate of glaſs was introduced between them, they were ſuſpended; but, on its being taken away, they were renewed.

This experiment anomalouſly did not ſucceed with every piece of fleſh; and never with warm water or ether. From accurately obſerving theſe facts, Mr Humboldt concludes, that freſh animal matter is ſometimes in a ſtate in which an inviſible conducting atmoſphere emanates from it; the limits of whoſe activity gradually decreaſes, in proportion to the time elapſed ſince its ſeparation from the body.

Mr Humboldt now proceeds to explain a very ingenious method of exhibiting the reſult of theſe experiments, in conciſe formulæ, by means of letters and ſigns, as in algebra. He then gives ſome cautions, reſpecting too haſty concluſions about negative experiments; and notices ſome circumſtances by which their ſucceſs is modified. As the degree of excitability influences the reſult, with the ſame mode of applying the coatings; ſo, with the ſame excitability, the mode of application varies the effect. It is greateſt when the connecting metal is firſt applied to the muſcle, and then to the nerve. This our author particularly obſerved on himſelf, when he galvaniſed parts from which the cuticle had been removed by bliſters; and he found the difference more remarkable, in proportion to the degree of irritability. Another important circumſtance is, that the contractions are ſtrong, in proportion to the ſize of the muſcular coating, but are not influenced by that of the nervous coating. No organ is capable of exhibiting the galvanic phenomena, that is not furniſhed with both irritable and ſenſible fibres. At leaſt Mr Humboldt tells us, that he was always able to diſcover a remaining nervous fibre, where there were contractions.

The next ſubject that engages our author's attention, is the conſideration of the ſubſtances, which in many caſes are neceſſary for the production of the galvanic phenomena. Where theſe ſubſtances are neceſſary in lower degrees of excitability, their activity depends both on their relative poſition, and on their individual nature. Their diſtinction into exciters and conductors, Mr Humboldt thinks inacurate, as every conductor acts, in certain circumſtances, as an exciter. He therefore denominates them connecting ſubſtances, or links of the firſt and ſecond claſs. The latter do not act in low degrees of excitability, unleſs intermixed in certain arrangements with the former; *i. e.* the metals and ſubſtances containing carbon. Mr Humboldt conſiders theſe ſubſtances ſingly; but as their relative activity has principally occupied thoſe who have before him attended to this ſubject, he only notices what appears to him to be new.

Metals. For tables ſhewing their reſpective activity, he refers to Pfaff's work; but obſerves, that the degree of excitability is as material a condition in them, as temperature in tables of affinity.

Carbonated and Sulphurated Metals, are likewiſe active. At firſt Mr Humboldt thought that the magnetic fluid was a ſource of heterogeneity in iron; but he found his error aroſe from the different quantities of carbon contained in the ſpecimens employed; and obſerves, that, hitherto, the effects of magnetiſm, on the living animal body, have not been rendered obvious to the ſenſes.

Mineralized Metals. The difference between the active and inactive ores, our author imagines to depend upon the ſtate of oxydation.

Oxyd of Mangneſe, is the only oxyd which is a perfect conductor of the galvanic fluid; but he rather thinks this power muſt be aſcribed to the carbon, which many varities of it contain.

Volta firſt diſcovered the exciting property of *Charcoal*, and Laghi that of coaks. There are, however, ſome coals which poſſeſs the ſame power naturally. And our author thinks, that when they do not, it is becauſe the carbon is not free, but enveloped with hydrogen; and that the ſame is the caſe with imperfectly charred wood. The exciting power of black lead is ſo great, that the ſmalleſt quantity affords a very ſtrong ſtimulus. Even lines drawn upon an ivory plate were ſufficient.

Touchſtone, too, our author found to act as an exciter; and he diſcovered carbon in its compoſition.

Aluminous and Vitriolic Schiſtus, alſo contain carbon enough to be conductors.

Water and other fluids, oil excepted, are perfect conductors. The ſolutions of all ſalts, acids and alkalies, alcohol and ethers, are active. Pure oil is inſulating; but, when converted into fluid ſoap, it conducts, nay, more ſtrongly than pure water. Dry ſoap, on the contrary, inſulates. The power of metals and carbon, of acting as galvanic exciters, ſeems to be deſtroyed by their being united with oxygen or hydrogen. But theſe ſame elements, united with each other, and with other inſulating ſubſtances, form perfect conductors. Hence, the power does not ſeem to depend on the properties of the elements, but is an individual peculiarity of the compound. Heat does not alter the conducting power of metals, but its action on water is ſtriking. Ice inſulates, water conducts; but ſteam, and every ſubſtance converted into an elaſtic fluid, again inſulate. Heating exciters to a red heat, does not rob them of their conducting power; nor does glaſs, by that means, acquire it. Melted wax and ſulphur ſtill inſulate.

Treating of the comparative conducting power of vegetable and animal ſubſtances, Mr Humboldt obſerves, that the epidermis of all parts of vegetables inſulates completely.

pletely. By ſtripping it off, they become conductors; but ſtill incomparably inferior to animal matters. He found roaſted, boiled, dried fleſh, ham, all more active than the juicieſt part of an apple. From which he concludes it probable, that animal and vegetable fluids are better conductors, in proportion as they are more *vitalized;* that is, the leſs their elements are mixed according to the known laws of chemical affinity. The teſt of the degree of this vitality, is the rapidity with which they change their form of mixture, when they ceaſe to make a part of the living whole.

In making experiments on galvaniſm, where he employed a chain of ſeveral perſons, Mr Humboldt has ſometimes found one who was a non-conductor. But as at other times they were conductors, and as Mr Hogg obſerved nearly the ſame thing with regard to the gymnotus electricus, he concludes, that ſome people, either always, or only in certain circumſtances, are inſenſible to the influence of the gymnotus, and are non-conductors of the galvanic fluid.

Teeth, which, in their natural ſtate, are non-conductors, are capable of producing the metallic taſte in Volta's experiment, or the flaſh of light in George Hunter's, when ſet on edge by an acid.

The chemical analogy which exiſts between ſome of the muſhroom-tribe and animal matters, naturally led our author to inveſtigate their conducting power. And he found that thoſe fungi, which in a ſtate of putrefaction emit a cadaverous, ſweet, animal ſmell, are equally good conductors in the galvanic chain as real animal ſubſtances.

TABLE *of the* POWER *of* SUBSTANCES *to Conduct the* GALVANIC FLUID.

CONDUCTORS.	NON-CONDUCTORS.
Metals. Sulphurated metals, and ores containing unoxydated metals.	*Oxyds of Metals.* Sulphats of metals, and ores containing oxydated metals.
Carbon. Vegetable and mineral coal, black lead, Kohlenblende.	*All Stones* not containing carbon.
Touchſtone, from the Nailaer mountains. Schiſtus aluminaris and carbonarius; grey and black ores of manganeſe.	*Animal Bones* in their natural ſtate.
Muſcular Fleſh. Animal membranes, nerves, ſinews, and veſſels; freſh, roaſted, boiled, and dried.	*Animal Hair.*
Teeth, ſet in edge by means of acids.	*The hardened White of Egg.*
Morels, and Muſhrooms, which undergo the putrid fermentation.	*Amber.*
White of Egg.	*Leaves and Stalks of Vegetables,* covered with the epidermis.
Water, Blood, Juices of Plants.	*Veſſels of Plants.*
Parts of Vegetables, with freſh cellular ſubſtance, and without epidermis.	*Wax.*
Spirits, Wine, Beer, Acids.	*Oil, Reſins, Gums.*
Solutions of Alkalies.	*Glaſs,* even warm.
Freſh Soft Soap.	*All Dry Salts.*
	All Elaſtic Fluids, dry and moiſt.
	Flame.
	Vacuum.

The galvanization is not interrupted by dividing the nerve, provided its parts be connected by ſome conducting ſubſtance. And Mr Humboldt found, in repeating this experiment, that it is not even neceſſary that the animal organ ſhould be an immediate link of the chain. Where the nerve is coated indirectly, the diſtance of the coating from the nerve ſeems of conſequence: but, on the contrary, where the coating is immediate, the length of the chain may be arbitrarily increaſed. Nor was the time of the paſſage of galvaniſm through one of 300 feet perceptible. Galvaniſm may likewiſe furniſh us, according to our author, with a means for deciding whether any ſubſtance reſemble animal matter in the nature of its compoſition, and upon the homogeneity of metals. The form of the exciters is alſo of conſequence. Two pieces of gold were connected by an highly poliſhed, ſquare, ſteel-rod. When the flat ſide was in contact with the gold, they remained inactive; but as ſoon as they were united by its edge, they brought forth convulſions. In leſs excitable animals, a ſlight concuſſion, whether it be of the metallic exciters, or confined to the galvanized animal matters, promotes the production of the phenomena.

It is difficult to decide, whether the ſtimulus continue the whole time the metals are in contact, or be confined to the moment of each new contact; for the ſtimulus may exiſt, though it ceaſe to act. Mr Humboldt rather thinks, that the contractions and ſenſations remain, after the chain is completed. This he was particularly ſenſible of, in galvanizing two ſores made by bliſters on his back. When the galvanic chain is already completed, uniting it a ſecond time, by a homogeneous or leſs powerful exciter, cauſes no effect; but a ſecond union, by one more powerful, renews the contractions. We have already ſeen, that the paſſage of the galvanic fluid produces no obvious effect on inanimate connecting links; but excitable links are affected by it, although its action ſeems to be more powerful on thoſe immediately connected with the coatings, than on thoſe more remote from them. Ligatures upon the nerve do not impede the galvanic phenomena, when that part of it, between its inſertion into the muſcle and the ligature, is not in contact with any conducting ſubſtance. If a nerve be tied, and its remote end coated with zinc, by connecting the zinc, by means of ſilver, with any part beyond the ligature, no contractions follow; but, if the connection be made with a part within the ligature, they take place as if there were no ſuch ligature. But if the part within the ligature be covered with a conducting ſubſtance, or the ligature be made cloſe to the inſertion of the nerve into the muſcle, there will be no contractions. From one experiment, Mr Humboldt thinks the diſtance of the coating from the ligature of conſequence. An inch-long crural nerve of a frog was tied with a hair, cloſe to its inſertion. When the coating of zinc was at the extremity, there were no contractions on applying the exciter beyond the ligature; but on moving forward the zinc to within one and a half line of the ligature, they occurred. Ligatures tied upon arteries always diminiſhed, and at laſt totally deſtroyed, the excitability of the muſcles to which they belonged.

Our author now proceeds to detail thoſe curious experiments, which, he thinks, prove the exiſtence of the power which animal matters have of acting, though not in actual contact. When a nerve is divided longitudinally, if a ſingle fibre be left connecting the coating with the muſcle, galvaniſm is not in the leaſt impeded. We have already ſeen, that if the ends of a nerve, divided acroſs, be connected by a conducting ſubſtance, the galvanic fluid paſſes equally readily as before; but it alſo paſſes, tho' the divided ends of the nerve be removed to a certain diſtance, and no connecting ſubſtance be uſed. The experiments proving this require much dexterity, to obviate all doubts with reſpect to their concluſiveneſs. Mr Humboldt, therefore, details at length his manner of performing them, and the precautions he took, that no connecting conductor ſhould poſſibly remain. He divided the nerve when ſuſpended in the air, and he ſupported both ends, as he lowered them gently, on a plate of glaſs, ſo that they remained about a line diſtant from each other.

If this be quickly accompliſhed with a tolerably excitable ſubject, contractions are produced by connecting the coating of the ſeparated portion of the nerve with the attached portion, or even with a lower part of the ſeparated portion itſelf, by a heterogeneous exciter. The ſeparated portion, therefore, propagates its energy through the intervening atmoſphere; nor is it neceſſary that the apertures of the divided canals be oppoſite to each other, for the experiment ſucceeds equally, when the ſide or any part of the nerves are within the diſtance of action. As the excitability of the organ diminiſhes, connecting the nervous coating with the ſeparated portion is no longer adequate to produce contractions; and the diſtance of the divided ends muſt be gradually leſſened, even when the connection is made with the attached portion, till at laſt they muſt be in contact. The greateſt diſtance, at which Mr Humboldt has been able to produce contractions in this way, was 5-4ths of a Paris line; and they never laſted longer than eight minutes. It is not neceſſary that the ſeparated portion be a part of the ſame nerve; for with pieces taken from the Rana eſculenta and Bufo, he excited the muſcles of the Rana temporaria and Lacerta agilis; but he could not, with the nerve of a mouſe, ſtimulate the cold-blooded animals.

From many experiments, Mr Humboldt thinks, the power of acting at a diſtance, belongs, excluſively, to the ſeparated portion; for he has ſeen, that after the contractions have entirely ceaſed, they re-appeared, by merely employing a freſh piece of nerve. Beſides, when he has endeavoured to augment the excitability of the parts, by moiſtening the attached portion with alkaline ſolutions, or oxymuriatic acid, he always failed; but he has been able to re-produce the contractions after they had ceaſed, by wetting the detached portion with oxymuriatic acid. If the divided ends of the nerve be ſupported in a very excitable ſubject, by bits of glaſs, ſo as to remain ſuſpended in the atmoſphere, the contractions ceaſe as ſoon as a plate of glaſs is interpoſed between them, though it touch neither of them. On taking it away, they return; on ſubſtituting for it a metal plate, they continue; but ceaſe, if it come in contact with the ſeparated portion. In warm-blooded animals, this experiment rarely ſucceeds, from the rapidity with which they loſe their excitability. All theſe facts prove, according to Mr Humboldt, the exiſtence of a ſenſitive or ſtimulating atmoſphere being expanded around every nerve. And he ſhews how it may be applied, to explain the univerſality of touch and taſte, in their reſpective organs, the phenomena attending the re-production of nerves, and thoſe caſes of ſympathy where there is no actual nervous communication. Laſtly, in a few caſes, Mr Humboldt produced contractions, by approaching the exciter within about a line of the muſcular organ; but the phenomena of the exiſtence of an active atmoſphere, are much ſeldomer viſible around muſcle, than around nerve.

For the production of the galvanic phenomena, the organical connection of nerve with the irritable fibre is abſolutely neceſſary; for a piece of ſeparated nerve never excites contractions in a muſcle, when it does not act on a nerve ſtill connected with the muſcle.

Increaſe of temperature does not affect the inſulating power of glaſs, amber, or wax; but increaſes the exciting power of metals, when it promotes the evaporation of a fluid from their ſurface. But Dr Wells has diſcovered, that friction with ſilk, wool, fiſh-ſkin, ſealing-wax, wood, marble, or the hand, communicates to homogeneous metals the power of exciting contractions, where they were previouſly inactive. And it is remarkable, that when the friction was made equally, or nearly equally, on both ends of the exciter, its effect was deſtroyed, or much leſſened. Mr Humboldt alſo expreſſes his ſatisfaction, that Dr Wells ſhould, at the ſame time with himſelf, have found the influence of moiſture on the galvanic phenomena, (although he believes him wrong, in ſuppoſing that its quantity is of any conſequence), and the manner of increaſing the exciting power of metals, by having placed them in contact with another. Mr Humboldt found, that where homogeneous gold could not produce contractions, by ſtriking it upon another piece of gold, it was not rendered active ; but a blow upon iron endued it with a weak, and upon zinc with a ſtrong, exciting power. It was indifferent whether the blow was given with the edge or whole ſurface. Mr Humboldt aſcribes the diminution of effect, when he performed the galvanic experiment in carbonic acid gas, and its increaſe in oxymuriatic gas, to their action on the excitability of the organ, not to any degree of conducting power; for he did not perceive any difference when it was performed inſtantly in oxygen, nitrous, hydrogen, azotic, carbonic acid, oxymuriatic, or carbonated hydrogen gaſes, before the excitability could be acted upon by them. Aldini found the contractions weaker in the Toricellian vacuum, and ſtronger in compreſſed air; but the animal organs retained their augmented or diminiſhed excitability, after they were taken out of the apparatus; which proves that the denſity of the air had acted on it alone. In all liquids, except oil, the galvanic experiment preſents the uſual reſult; or the ſlight variations are eaſily explained, from the known properties of the medium.

In the preceding obſervations, Mr Humboldt has conſidered galvaniſm without any reference to the nature of the animal to which the ſtimulus was applied. He next proceeds to the inveſtigation of that ſubject; and has intermixed it with many important obſervations, relating to comparative phyſiology and anatomy.

He has never been able to diſcover any effects of galvaniſm upon vegetables; but he is cautious of concluding from it, that irritability is independent of nervous energy, and adduces a number of reaſons, why its effects, even if vegetables be endowed with a nervous ſyſtem, ſhould not be obvious.

Mr Humboldt ſucceeded perfectly in galvanizing a great variety of worms and inſects, and his experiments led him to make many curious remarks on their anatomy and phyſiology, eſpecially with regard to their nervous ſyſtem. Fiſhes were convulſed with particular violence. Mr Abilgaard galvanized the torpedo at Naples; but obſerved nothing extraordinary. Amphibious animals have been moſt generally the ſubjects of theſe experiments. Their accumulated irritability, on awakening from their winter torpidity, Mr Humboldt does not think explicable on the common opinion of an accumulation of oxygen; becauſe, during that ſtate, leſs of it is ſupplied to the ſyſtem, and though, from the want of motion, leſs is alſo conſumed, yet want of motion in itſelf diminiſhes irritability. Mr Humboldt aſcribes it to the abſence of ſtimuli, and diminution of the action of the ſenſorial power. He thinks it not unphiloſophical to ſuppoſe, that fibrous motions, and ſome chemical change, may take place in the organ of the ſoul, at the ſame inſtant with thought. What becomes of the immenſe quantity of oxygen, of which the blood is robbed, during its paſſage through the brain? Is the caloric which is ſeparated, recombined into an elaſtic fluid, or ſomething analogous to it? The anſwers to theſe, and many ſimilar queſtions, our author wiſely leaves to poſterity. Birds, he obſerves, have the moſt extenſive reſpiration, and the greateſt degree of animal heat; but in them, the galvanic phenomena are moſt tranſitory. He repeated many of his experiments on various ſpecies of the claſs mammalia. In George Hunter's experiment, he obſerves, that the iris is not affected. This, as well as Volta's production of taſte by two metals, is rendered much more obvious, and may be produced by homogeneous metals, if the mouth be wetted with a weak ſolution of cauſtic potaſh. Galvanizm has not yet been brought to affect the ſenſe of ſmell. Mr Humboldt produced a diſpoſition to inflammation in the cavity from which a tooth had juſt been drawn, and in a wound in his hand. By galvanizing two wounds, purpoſely made with bliſtering-plaſter on his own back, the white mild ſerum was, in a few minutes, converted into a red fluid, which inflamed the parts over which it ran; the cucularis muſcle ſwelled very much, and its contractions were continued even to its upper inſertions. The painful ſenſation produced, had no reſemblance to that cauſed by electricity. By galvaniſing the mouth and rectum, Achard ſaw pains in the abdomen, increaſed action of the ſtomach, and a change in the excrements, take place. By the ſame proceſs, Mr Humboldt reſuſcitated birds apparently drowned; and he thinks that it might be employed with advantage, in caſes of aſphyxia. Experiments have been made on amputated extremities, by Gentili, Creve, and Starck; and the following letter by Dr Grapengieſer is too remarkable to be omitted.

"Amongſt the many medical curioſities "of Dreſden, there is a patient in the military hoſpital who deſerves particular attention. This unfortunate man had long "had a ſcrotal hernia. It accidentally became ſtrangulated, and was treated with "emollient applications. Violent ſuppuration was the conſequence, with an opening in the hernial ſac; out of which now "hang a part of the great inteſtine, the "coecum and colon tranſverſum, and a part "of the flexus iliacus. They are inverted, "ſo that the external ſurface has now become the internal. When he ſits, they "hang down to his knee; and at each ſide "there is an opening. From the one, injections, when he receives any, are diſcharged; and by the other, the fœces "are voided. Without a drawing, it is

" difficult to comprehend this wonderful ſi-" tuation of parts. Between the colon and " ſmall inteſtine, there is a thick, hard ring, " which ſeparates the two parts exactly, and " binds the whole convolution ſtrongly to-" gether. This is probably the valvula coli; " which, by the inverſion of the inteſtines, " is much enlarged, and has loſt its form. " I propoſed immediately to try the effects " of galvanization on this ſubject. The " patient conſented readily; and, from " the many experiments you had already " made upon yourſelf, I was not afraid of " any danger. I coated a part of the in-" teſtine with ſilver, and another with zinc. " Scarcely were they brought into contact, " when the periſtaltic motion became much " increaſed, though mechanical irritation " acted very ſlowly, and only when violent-" ly applied. On the application of the " metals, on the contrary, the undulations " were frequent and lively. The patient " complained of a burning ſenſation in the " coated parts. As in the wounds on your " back, the flow of ſerum was increaſed; " here, too, the activity of the mucous "glands and exhaling veſſels ſeemed to be "augmented by galvanization. In a few "minutes it was ſo great, that large drops "ran from both metals. In imitation of "your lateſt experiments on nervous action "with the alkalies, I moiſtened the ſurface "of the ſmall inteſtine with a ſolution of "potaſh; and to my aſtoniſhment, with the "ſame coating, the periſtaltic motion was "increaſed ſixfold. The patient alſo felt "the burning more acute, which is to be "aſcribed to the increaſed irritability alone, "for the potaſh of itſelf produced only a "very weak and tranſitory ſenſation. I "could not abuſe the man's goodneſs any "more, by proſecuting my experiments." Here is the moſt direct evidence of the action of galvanizm on the involuntary muſcles of the inteſtines; and our author brings many ſatisfactory proofs, in confirmation of thoſe already adduced by Fowler, Schmuck, and others, to demonſtrate that the heart is alſo ſubjected to its influence, and that it is furniſhed with nerves, in oppoſition to the celebrated opinion of Behrends.

No ſooner were a few experiments made,

with regard to galvaniſm, than it gave riſe to theories. Galvani and Aldini, at the head of the Bologna ſchool, aſcribed to the outer ſide of the muſcular layers a negative, and to the inner ſide and medulla of the nerves, a poſitive electricity; and conſidered the muſcular fibres as a ſeries of parallel Leyden phials. Valli's firſt theory was very analogous to this; and his laſt is too abſurd to be noticed. But our author conſiders as by far the moſt ingenious theory that has yet been formed on this ſubject, that of Alexander Volta, Profeſſor at Pavia. As it has been publiſhed only in a very imperfect and mutilated ſtate, Mr Humboldt details it anew, as he had it from Volta himſelf.

All ſolid and fluid matters have, according to their attraction for electricity, abſorbed more or leſs of it, and are therefore weakly and unequally charged. When they come in contact, they endeavour to arrive at a ſtate of equilibrium; but, before this, the electricity was accumulated in them in different degrees. Moiſt ſubſtances, or conductors of the ſecond claſs, emanate leſs electricity than metals, or conductors of the firſt claſs. Amongſt heterogeneous metals, there is alſo a difference; but it is moſt remarkable in their compariſon with bodies of the ſecond claſs. The charges of metals, compared with each other, are almoſt equal; at leaſt, their difference when they are in contact is not regarded in the theory. When only two bodies, one of the firſt, and one of the ſecond claſs, are in contact, no free circulation of the electric fluid can take place. Suppoſe a moiſt animal ſubſtance, *a*, in contact with a homogeneous metal, *b*; and that *a* emits electricity from both ends, with a force as 2, and *b* with a force as 5; there is no reaſon why the electric fluid ſhould flow rather in one direction than in another, the forces at the points of contact being equal.

When more than two bodies of the firſt and ſecond claſs are in contact, the electric fluid is forced to circulate in one or other direction. Suppoſe three ſubſtances [zinc, *b*; ſilver, *c*; and animal matter, *a*;] in contact; and that the ſilver emits the electric fluid with a force as 3; the zinc as 5;

and the animal matter as 2; the fluid muſt force its way from *a*, through *c*, to *b*; becauſe at the mutual point of contact of the metals, the forces are nearly equal, and their difference may be diſregarded. But the animal matter is ſubjected to the action of unequal powers; of that of ſilver, as 3, and of zinc, as 5. The electric fluid muſt, therefore, circulate from *a*, to *c*, to *b*. If we had electrometers ſufficiently ſenſible to indicate this momentary paſſage of the electricity, we ſhould be convinced, that ſuch a circulation, produced by diſturbed equilibrium, actually exiſts, when a piece of wet ſponge is brought in contact with heterogeneous metals. But our inſtruments are too coarſe to ſhew theſe changes in dead matter. The irritable animal fibre is the only electroſcope ſufficiently fine to indicate it. And therefore, when an irritable organ, as the thigh of a frog, is uſed for *a*, its muſcles are excited to contraction by the circulation of the electricity.

Mr Humboldt next proceeds to confute this very ingenious theory. As it is no longer maintained, that the fluid is really electrical, but only that it is analogous to electricity, it would be ſuperfluous, at preſent, to examine that point. The other conditions of the theory, Mr Humboldt thinks ſufficiently refuted by his diſcovery, that the galvanic phenomena may be produced without any chain-like connection; by the poſſibility of exciting contractions, where there are only two ſubſtances concerned; and by many other poſitive caſes, which, according to Volta's hypotheſis, ſhould be negative.

The moſt important concluſion that follows from our author's experiments, is, he thinks, that the ſtimulus in the galvanic phenomena exiſts in the excitable organs themſelves, and that the metals and other matters ſometimes forming links in the galvanic chain, are only neceſſary in caſes of low excitability. They ſeem to increaſe the ſtimulus, but are not its actual ſource.

The moſt general expreſſion for the conditions under which the galvanic phenomena occur, is thus worded by our author: "What "iſſues out of the nerves, ſeems to act upon "them as a ſtimulus, when it returns into "them."

Mr. Humboldt has drawn up the following table, from his numerous experiments, as the conditions which ſeem neceſſary for the production of the galvanic phenomena, with different degrees of excitability.

They ſeem to be produced with the firſt or higheſt degree, without a chain.

2. When the exciter, ſurrounded with muſcular fleſh, acts without being brought into contact with the nerve.

3. When organically connected parts are brought into immediate contact.

4. When they are connected by animal conductors.

5. By perfectly homogeneous metallic bodies, or thoſe containing carbon.

6. By heterogeneous metals, not in immediate contact.

7. By homogeneous metals, between which a heterogeneous one, moiſtened on both ſides with an evaporating fluid, lies.

8. By a chain, of which the ſenſible organ does not form a link, but is merely mediately connected.

9. By heterogeneous metals in immediate contact.

10. By homogeneous metals, and one heterogeneous,

terogeneous, one ſide of which is moiſtened with an evaporating fluid.

11. By ſtriking the heterogeneous metals on each other.

12. By connecting the coating of the nerve with the muſcle, not with the nerve.

13. By applying the conductor firſt to the muſcle, and afterwards to the nervous coating.

14. When the portion of nerve, between its coating and the muſcle, paſſes through the atmoſphere, and the muſcle itſelf is not in contact with the nervous coating.

15. When the tired nerve is ſtretched or ſpread out upon its coating. And,

16. When the muſcular organ is laid open, and a bared nerve is touched by the ſilver conductor.

Mr Humboldt ſuſpects he may have erred in the higheſt ſteps of the ſcale, and that the ſecond ought to be firſt, and the firſt third; but, in the lower ones, he is convinced of his accuracy. He has not, in it, taken any notice of the chemical nature of the exciters and conductors, becauſe that ſubject has been already ſo fully treated by others.

Having thus enumerated the various conditions, under which the phenomena are obſervable, he now proceeds to ſtate his own theory, which is too ingenious to be omitted, although the analyſis of this curious work be already extended to an unuſual length.—His opinion is, that the whole phenomena depend on the hinderance given by imperfect conductors of the galvanic fluid to its paſſage, and that the intenſity of the effect is in exact proportion to the greatneſs of the hinderance. He, beſides, imagines, that the galvanic fluid, which is accumulated in the animal organs, paſſes more eaſily through animal matter, than through metals; more eaſily through homogeneous, than heterogeneous metals. Upon theſe principles, he endeavours to explain all the phenomena.

He thinks it proved, that in organic bodies, ſo long as they continue excitable, there is a certain fluid contained, and that it exiſts in unequal quantities in the muſcle and nerve in their natural ſtate. This inequality is eaſily conceivable; for the parts concerned are endued with life. In them, new combinations and decompoſitions are going on continually; and as this chemical vital proceſs is differently modified in each organ, and as the galvanic fluid ſeems to be a principal agent in it, it follows, that in every given portion of time, a greater or leſs unequal diſtribution of galvaniſm takes place in the nerve and muſcle. If the extremity of a nerve be bent back againſt itſelf, or if the thigh of a frog be brought into contact with its iſchiadic nerve laid bare, but ſtill ſurrounded with conducting animal ſubſtance, no effect will follow, becauſe the organic connection and ſurrounding conductors prevent any very unequal diſtribution of galvaniſm from taking place. If, however, the nerve be freed from all conducting matter, and remain in contact with the air alone, contractions follow; for now the galvaniſm, which was before conſtantly abſorbed from it, becomes accumulated, and a diſcharge takes place, on bringing the nerve and muſcle into contact. The longer the nerve remains bare, the leſs the difference becomes; and the more remote the inſulated part of the nerve is, the longer is it before equilibrium ſucceeds. Hence, bringing the crural muſcle in contact with its own nerve, never produces contractions, as it does on touching the more diſtant iſchiadic nerve.

This theory conſiders the phenomena of galvaniſm as the peculiar effects of vitality. It ſhews us the poſſibility of volition, producing muſcular motion, in the ſame manner as the application of the metals; by ſuppoſing that, at the ſame inſtant with the ideas of volition, a proceſs occurs in the animal machine, by which either more galvanic fluid is ſeparated in the nerves, or conducted by them from the brain. The contractions will be ſtrong, in proportion to its quantity, and will continue as long as it continues. The phenomenon of turgeſcence, or muſcular contraction, may be conſidered as the conſequence of a chemical change of mixture, as the conſequence of the power of attraction acting without hinderance. The principles or elements conſtituting the muſcular fibre, have, like all other matter, a tendency to combine more intimately, or to come into cloſer union.

The individual attractions of theſe principles are variouſly modified by numerous cauſes, and mutually circumſcribe each other. On this ſelf-limitation, the relaxed or contracted ſtate of the muſcular fibre depends. As often as there is a change of mixture, whether an old ſubſtance oppoſing the mutual attraction of others be ſeparated, or a new matter increaſing it be added, the principles come into cloſer union, and the muſcular fibres become ſhorter. Since many facts ſhew, that by muſcular motion the blood is deoxygenated, and as the galvanic fluid is a chief agent in muſcular motion, Mr Humboldt ſuppoſes, that by its mixture, the union of the hydrogen and azot with the oxygen is promoted, and that thus theſe elements of the fibres are condenſed. But the ſhortening of the fibres can only be momentary, becauſe the oxydated principles, condenſed by the galvanic fluid, are again organically ſeparated, and their place is ſupplied by new oxygen, azot, and hydrogen. Each contraction is therefore ſucceeded by relaxation. In this manner, Mr Humbolt continues to explain diſeaſed muſcular motion, and the phenomena of galvaniſm. He then recapitulates the chief poſitions of his theory, which he thinks reſt upon ſimple notions of attraction, impulſe, hinderance, velocity, and power.

1. The ſtate of the muſcular fibre, in regard to relaxation or contraction, is determined by the degree of attraction which its elements exert on each other.

2. Every change of mixture which occurs in the organs, modifies this power of attraction. A ſudden modification produces contraction.

3. In the living nervous and muſcular fibre, a fluid is ſeparated, which can flow from the former into the latter, ſince they are organically connected.

4. A ſudden flow, cauſed by unequal accumulation, changes the power of attraction of the elements, and is a chief cauſe of fibrous contraction.

5. In voluntary muſcular motion, at the ſame inſtant with the idea of volition, the organic ſecretion of that fluid in the brain or nerves, ſeems to be ſuddenly increaſed,

and to cauſe a diſcharge or ſudden flow into the muſcle

6. When a nerve is ſo prepared that it is ſurrounded only by an inſulating medium, air, its charge is augmented, and differs from that of muſcle. This difference decreaſes gradually, becauſe the nerve is inſerted into the muſcle, and endeavours to get into equilibrium with it.

7. If, therefore, before the decreaſe of this difference, before the equilibrium is reſtored, the muſcle be brought into contact with the nerve, a contraction will follow, and be more violent, the more diſtant the nerve and muſcle are from each other.

8. If a conducting communication be made from one part of the nerve to another, the galvanic fluid, attracted by the conductor, will endeavour to paſs through, and as it meets hinderances to be overcome, will accumulate. During this accumulation, the vital principle continues to ſecrete new galvanic fluid into the organ. When, at laſt, it forces its way, and the accumulated galvaniſm returns, ſudden ſuperabundance and contraction will ariſe.

9. The greater the hinderance, and the later the diſcharge is of happening, the greater muſt its effects be. The galvanic fluid ſeems to paſs eaſily into animal matter, more difficultly into metallic, and moſt difficultly from one metal into another heterogeneous to it. If the muſcular contractions are, therefore, to occur as ſtrongly where there is a leſs degree of excitability, as where there is a greater, the links of the conducting chain muſt be ſo diſpoſed, that the hinderance to the paſſage of the fluid muſt be increaſed.

10. As each connecting link communicates with the organs in two points, there muſt be two ſtreams in oppoſite directions. If they be equal, or nearly ſo, the effect will be weaker; becauſe the one meets the other, the metals are more quickly penetrated, and the diſcharge occurs ſooner.

11. If the one ſtream be much weaker than the other, it will not only proceed more ſlowly to its ſupport, and thus increaſe the effect, but alſo, when the diſcharge takes place, be carried with it; and thus will

the ſtimulus be more concentrated in one point.

12. All theſe mechanical relations do not exclude other acceſſory, chemical cauſes. On the contrary, it is highly probable, that theſe, ſuch as the ſpecific electric charges of the metals, their temperature, influence on the evaporation and decompoſition of liquids, modify the hinderances which determine the galvanic fluid to a forced paſſage.

Although Mr Humboldt has uſed the term galvanic fluid, as being convenient, he thinks its nature, as well as that of caloric, light, electricity, and magnatiſm, hitherto perfectly unknown. In its phenomena, galvaniſm ſeems to have the greateſt reſemblance to electricity; and their identity is ſupported by Galvani, Valli, Aldini, Volta, Vaſſali, Carradori, and Darwin; but denied by Fowler, Cavallo, and Creve. The arguments for their identity may be reduced to four.

1. In violent muſcular motions, the animal body ſhews evident marks of uncombined electricity.

2. Galvaniſm is, like electricity, inſulated by glaſs, oil, roſin, and ſealing-wax; and conducted by bodies containing metals, charcoal, and moiſture.

3. The taſte, in Volta's tongue-experiment, is very analogous to that produced by a ſtream of electricity.

4. They both ſtimulate the nerves, ſo as to produce muſcular contractions, after they are not excitable by any other ſtimulus.

Of theſe, Mr Humboldt conſiders the ſecond as the only one of conſequence; and endeavours to overturn it, by the following objections.

1. Old, well dried, bleached bones, are as good, and in part better, conductors of electricity, than the metals; but inſulate galvaniſm, as perfectly as glaſs or ſealing-wax.

2. Rarified air conducts the weakeſt degree of electricity, but not galvaniſm.

3. Flame is the moſt perfect conductor of electricity, but inſulates galvaniſm.

4. Warm glaſs conducts electricity perfectly, but inſulates galvaniſm.

As theſe facts completely overturn the idea of identity, Mr Humboldt next examines, whether they be modifications of each other; and, with this view, inquires into the nature of electricity and magnetiſm; with which he ſhews that we are ſo little acquainted, that the queſtion only amounts to aſking, whether one unknown ſubſtance be a modification of another equally unknown?

After explaining the manner in which the ſpecific nature of the metals, their influence on the evaporation and decompoſition of fluids, may vary the degree of hinderance, he adduces many arguments to overturn Creve's opinion, that "by applying "metals to animal matter, a chemical pro- "ceſs takes place, in which the metals de- "compoſe the water, and conduct at the "ſame time a quantity of caloric, which "unites with that rendered free during the "decompoſition of the water. The oxy- "gen then unites with the metal, and the "hydrogen with the caloric. By this laſt "combination, an electrical matter is form- "ed; which, as a product of the proceſs, "is really the proximate cauſe of the gal-

" vanic phenomena." " Gardini has proved, " that electrical fire conſiſts of hydrogen " and caloric."

Having ſtated a few more queſtions, which, in our preſent ſtate of knowledge, cannot be ſatisfactorily explained, Mr Humboldt concludes this very ingenious work, of which almoſt every page contains new ideas, with ſome conjectures concerning the atmoſpheres of living organs.

VII.

Joannis Chriſtiani Reil, *Med. et Chir. Doctoris, Therapeiæ Prof. Pub. Ord. Scholæ Clinicæ Directoris, Poliatri Halenſis, Acad. Imp. Nat. Cur. Artium Med. et Chem. Brux.; necnon Soc. Med. Helvet. Correſp. Sodalis, Exercitationum Anatomicarum Faſciculus Primus, de Structura Nervorum, Tribus Tabulis Æneis illuſtratus.* Folio Imp. Halæ, Saxonum, 1796.

IN this ſplendid work, the learned and ingenious Profeſſor endeavours to ſhew, how chemical agents may be employed in the proſecution of anatomical inquiries, in parts which from their minuteneſs have baffled the ſkill of the moſt dextrous anatomiſts. He has here given us a ſpecimen of his own ſucceſs, and points out to others ſome ſubjects, in the inveſtigation of which the methods he has made uſe of promiſe conſiderable aid.

The firſt chapter treats of the proper coat of the nerves, or Neurilema. The nerves conſtitute a peculiar organic ſyſtem, of a fibrous nature, ſome of whoſe proceſſes are immediately connected with the brain and ſpinal marrow, and may be termed Central Terminations; while others are diſtributed among the organs of the body, looſely and without any actual connection, its external or circumferential terminations. They conſiſt progreſſively of bundles, chords, and tubes, filled with medulla. Their proper coat, or neurilema, is an independent organ, not ariſing from the pia mater, or any other part of the body. It determines the organization of the nerves, and differs in ſize, becoming contracted or expanded, according to the degree of nervous energy, required by the adjacent parts. The limits between the nerves and brain, and ſpinal marrow, or their central terminations, are marked by its ceſſation; for the connection is made only by the medulla and external coat, the internal and minute tubulous ſtructure of the neurilema terminating a little before the union takes place. This is moſt remarkable in the optic nerves, which ceaſe to be tubular a little before their decuſſation, and this ſooner in their axis than in their circumference. The neurilema is compoſed of cellular membrane, and is largely ſupplied with lymphatic and ſanguiferous veſſels, which penetrate it every where. Though thin, it is firm and elaſtic, eſpecially in living animals. Its external ſurface is ſmooth, and very looſely connected with the cellular ſubſtance ſurrounding it.

By macerating a portion of nerve which does not ſend off branches, ſuch as the iſchiadic or median, for about twelve hours, in ſoap-lees, the nervous chords may be eaſily extracted, one after the other, from the cellular coat ſurrounding them. Theſe chords are of very various ſizes, even in the ſame bundle, and are compoſed of a number of very fine tubes of different diameters, in different nerves and animals, every where anaſtomoſing, and containing the medulla. The neurilema of many nerves retains this tubulous ſtructure to their terminations, eſpecially their central ones; while in others, as in the papillæ of the lingual and cutaneous nerves, it aſſumes a cellular and ſpongy ſtructure. If an optic nerve be cut off beyond its decuſſation, and removed along with the eye, the cornea cut open, and the whole be immerſed from ſix to twelve hours in ſoap-lees, and then gently preſſed between the fingers, in diſtilled water, which muſt be often changed, a milky fluid, conſiſting of the medulla diſſolved in the cauſtic alkali, will exude both from the cut-end of the nerve and the cribrous part of the ſclerotica, and, by repeating the operation, the whole medulla will be at laſt waſhed out, when the neurilema may be eaſily filled with air or mercury from either end, and its ſtructure rendered obvious.

The neurilema, according to our author, is not an accidental, but eſſential part of the nerves. It determines their conſiſtence, form, and organization. It ſupplies the continual waſte of medullary matter, which is topically ſecreted in every part of the nerves by the neurilema; and therefore the nervous power, depending on the quality and quantity of the medulla, is generated in the nerve itſelf, and does not proceed from the brain, as was formerly imagined. Every portion of nerve, even the ſmalleſt, lives of itſelf, is nouriſhed, and increaſes independently, and, by its own power, is irritable, and capable of performing its functions. That the nerves do not depend on the brain, but only cohere with it, is ſufficiently proved by acephalous monſters.

In the ſecond chapter, Dr Reil explains the nature and uſes of the cellular coat; and in the third he treats of the nervous chords. Theſe are beſt ſeen by macerating the bundle of nerves to be examined, for three or ſix days, in nitric acid, of the ſpecific gravity of 1,127, which renders the medulla hard, and of a fine yellow colour, while all the cellular membrane and neurilema is deſtroyed, and may be waſhed away by the affuſion of diſtilled water. In different nerves, theſe chords are very various with regard to number, ſize, and connection. In ſome there is only one, and they are uſually largeſt in the middle of the nerve, and become ſmaller at its extremities. In the optic nerve alone, the ſtructure is different. The nerves are ſenſible at both their central and external extremities, both of which are liable to very little variety.

The nervous threads or fibrils contained in the internal tubes of the neurilema, form the ſubject of the fourth chapter. To demonſtrate them well, a very freſh piece of nerve muſt be macerated for ſeveral days in muriatic acid, much diluted. Its extremities are then to be expanded, and the acid cautiouſly removed by a piece of ſponge, or a ſyringe, and replaced by plenty of diſtilled water. Each chord is then found to conſiſt of ſeveral of theſe fibrils, connected by the neurilema. Their ſize is very various, and they form frequent anaſtomoſes.

No part of the body is more copiouſly ſupplied with blood-veſſels, than the nervous ſyſtem. The nerves are always cloſely accompanied by arteries, which ſend twigs to be diſtributed throughout their whole ſubſtance. They muſt therefore be ſubject to inflammation; and our author actually found the nerves of a man, who died of typhus, and had had many violent nervous ſymptoms, remarkably turgid with blood.

After endeavouring to overturn the prevailing theories of nervous action, Dr Reil ſtates his own. He ſuppoſes it to ariſe from a change in the mixture of the medulla, a chemico-animal proceſs, agreeable to the theory of life which he has detailed in his phyſiological archives. The plentiful afflux of blood co-operates in this conſtant compoſition and decompoſition in the medullary ſubſtance, and ſupplies the waſte which muſt always happen there.

Dr Reil thinks that the functions of the nerves are not confined to ſenſation and volition alone; but that they ſerve other important purpoſes in the animal œconomy, not yet ſufficiently underſtood; that they co-operate in nouriſhing the body, and in the production of animal heat; that they are the medium connecting all the parts of the body, and the conductors of the vital power; and that they are equally neceſſary with the veſſels in the chemico-animal proceſſes, which, according to him, conſtitute life.

It is not probable, that there are nerves and medullary matter in every point where ſenſation, motion, and other effects depending on nervous influence, exiſt. This, and ſome other facts adduced by our author, have led him to attribute to them a ſphere of action extending beyond their material ſubſtance.

VIII.

Obſervations Sur la Nature, et ſur le Traitement du Rachitiſme. Per Antoine Portal, *Profeſſeur de Medicine, &c.* 8vo. Paris, 1797.

M. PORTAL, who has had very extenſive experience in cafes of rickets, exhibits in this work a view of the reſult of his practice in that diſeaſe.

Every caſe of mollities oſſium, he conſiders to be Rachitis, and conſequently, according to him, the diſeaſe occurs at every different period of life. He imagines that it is generally ſymptomatic, ariſing from ſome previous complaint, and he doubts entirely the exiſtence of idiophatic rickets. He divides the ſymptomatic diſeaſe into ſix ſpecies, experience, he ſays, having convinced him, that it is occaſioned by the following circumſtances, namely, the ſyphilic virus, ſcrophula, ſcurvy, eruptive diſeaſes, obſtructions in the abdominal viſcera, and rheumatiſm or gout.

1. Rickets in children ariſing from the ſyphilitic virus, are accompanied with the continual diſcharge of a greeniſh matter from the eye-lids, with chancres or ulcerations about the mouth, and ſometimes blotches about the genitals, and excoriations about the anus. The ſkin at the ſame time is of a dull yellow colour, and is dry, and rough to the touch. Emaciation very quickly takes place. Hectic fever, accompanied with frequent cough, ſupervenes, and conſtant vomiting, or colliquative diarrhœa, generally precede death. In ſome of theſe caſes, it has been found, not only that the ſpongy bones became enlarged, and the long ones curved, but alſo that ſome of the bones were harder, and more brittle than natural, and rather loſt than gained in ſize. In the ſame diſeaſe in adults, the ſpongy bones, and particularly thoſe of the noſe

and mouth, are moſt liable to become affected.

For the cure of this ſpecies of the diſeaſe, mercurial preparations are, according to M. Portal's opinion, alone to be truſted. Where the children are ſtill upon the breaſt, it was always ſufficient to give theſe remedies excluſively to the nurſe. A long continued gentle courſe, he conſiders in many caſes abſolutely neceſſary; and on every occaſion much preferable to the reverſe. In the exhibition of mercurials, great care muſt be taken, that no irritation be excited in the alimentary canal, and conſequently the doſes muſt be diminiſhed, and given at greater intervals, whenever ſymptoms of gripes or diarrhœa occur; and it is particularly adviſed, that the medicine be ſuſpended on the ſlighteſt appearance of ſalivation.

2. Children affected with rickets from ſcrophula, generally have the head conſiderably enlarged, chiefly from the increaſed ſize of the parietal bones. Their trunk is much bent forwards, and their back arched; the epiphyſes of the long bones are greatly

ſwelled; the clavicles extremely convex anteriorly, forming a regular hollow, poſteriorly. The humerus is generally bent inwards, and the bones of the fore-arm ſtill more ſo; the hands are large. The body of the thigh-bone is bent forwards, the condyles are nearly on a level, the tibia and the fibula, are frequently concave towards the outſide of the leg, the knees in walking preſs on each other, the feet are turned outwards; and when the child walks or ſtands, it reſts upon their ſide. The breaſt is ſmall and tight, the ribs are large, and ſeem as if bound together at their extremities next the ſternum. The pelvis is ſmaller, the ilia being narrow, and the pubis flattened. The bones of the pelvis are relaxed, which renders the gait unſteady, and ſometimes even prevents walking. Various deviations in the form of the ſpine, ariſe from changes produced upon the cartilages and ligaments which connect the vertebræ together.

In many caſes of this ſpecies of rickets, ſwellings, of the lymphatic glands are perceived, but our author does not conſider the appearance of theſe ſwellings as forming a ſure indication of the nature of the diſeaſe; for he has not only ſeen many inſtances of rickets from ſcrophula, unaccompanied with ſuch ſymptoms, but he has alſo frequently obſerved them in that ſpecies, occaſioned by the ſyphilitic virus. He admits, however, that it is extremely difficult to draw the line of diſtinction.

Mercurial preparations, joined with antiſcorbutics, together with a drain from iſſues or ſetons; but particularly from burning with moxa, and the occaſional uſe of mineral waters of a ſulphureous nature, ſuch as thoſe of Bareges, are the means to be employed in the cure of this ſpecies of rickets.

3. The third ſpecies of the diſeaſe, that ariſing from ſcurvy, is not nearly ſo frequent as the two former. The caſes where rickets are the conſequences of this cauſe alone, are ſufficiently ſtrongly marked, but it ſometimes happens, that, along with ſwelled gums, and that particular appearance of the ſkin peculiar to ſcurvy, the inguinal and axillary glands, and alſo thoſe of the neck, are enlarged; and hence there is the ap-

pearance of a combination of ſcurvy and ſcrophula.

Antiſcorbutics are the chief remedies recommended for ſimple caſes; and where there ſeems to be a complication of ſcrophula, a courſe of mercury is adviſed, after the uſe of the antiſcorbutics. M. Portal is of opinion, that in rickets ariſing ſolely from ſcurvy, mercurial preparations are injurious.

4. The fourth ſpecies of ſymptomatic rickets, according to our author's diviſion, is that ſucceeding to cutaneous eruptions. This ſpecies ſupervenes to the meaſles, the ſmall-pox, the itch, and even after the ſudden diſappearance of the red gum, or where that eruption has not been ſufficiently copious, or has not continued for a ſufficient length of time. M. Portal imagines, that the diſeaſe may alſo ariſe from the repulſion of any cutaneous eruption.

With reſpect to the treatment, he adviſes that a bliſter be immediately applied, as nearly as poſſible to the ſeat of the eruption, and that a drain from an iſſue be eſtabliſhed. Infuſions of the flowers of borage, or elder, or a decoction of ſarſaparilla, ſhould be frequently drank; if the ſymptoms do not render ſuch drinks improper, antimonial preparations are of the greateſt uſe. Aſſes milk, if there be no obſtruction in the abdominal viſcera, ſhould be preſcribed. Bathing in milk-warm water proves beneficial. M. Portal mentions, that he has tried in this, and ſome other ſpecies of rickets, preparations of ſal ammoniac, without ſucceſs. In two caſes where the diſeaſe ſucceeded to the itch, he cauſed the children to be re-infected, but without any benefit.

5. Rickets preceded by obſtructions in the abdominal viſcera are, according to our author, a very common occurrence. The liver is the principal ſeat of the obſtruction. In theſe caſes it is enlarged, but its texture generally continues natural, though ſometimes it is leſs firm. By its increaſe in ſize, it raiſes the diaphragm, compreſſes the right lobes of the lungs, rendering it more compact than natural, and injuring its texture, ſo that it becomes fleſhy or ſteatomatous. Difficulty of breathing, and watery effuſions into the thorax, are the conſequences. The enlargement of the liver

commences at ſome period, between birth and the eighteenth month, or ſecond year. The ſpleen is conſiderably enlarged, it is ſeldom hardened or ſhrunk; but this ſometimes happens, and when that is the caſe, the meſenteric veſſels connected with the vena porta, are extremely diſtended with blood. The affections of the ſpleen, and of the meſentery, however, our author ſuppoſes are ſecondary to that of the liver, becauſe in ſome caſes the liver alone is diſeaſed.

While theſe abdominal obſtructions are forming, and ſometimes after they have actually formed, ſymptoms of rickets, with hectic fever and emaciation, take place, and the child is gradually deſtroyed. M. Portal ſuppoſes, that the ſame circumſtances which produce this ſpecies of rickets, frequently deſtroy children during the proceſs of teething. He imagines, that obſtructions in the abdominal viſcera are occaſioned chiefly by bad diet, both of the nurſe and of the child.

The treatment conſiſts in the regulation of the diet, the exhibition of antiſcorbutics both to the nurſe and to the child, and in ſome caſes, a combination of mercurials with theſe, was found neceſſary. Where the proceſs of dentition is accompanied with much pain, the application of leeches to the gums, the eſtabliſhment of an iſſue in the arm, or keeping up a drain from behind the ears, together with warm bathing, are chiefly recommended.

6. Rickets ariſing from gout or rheumatiſm, attack adults only. This ſpecies of the diſeaſe is not ſo common, he thinks, as has been ſuppoſed. In caſes of this kind, ſometimes the bones are ſoftened, and ſometimes they are enlarged, and rendered deformed by an addition of oſſific matter.

In the treatment of ſuch caſes, our principal dependance is to be placed, we are told, upon bliſters or iſſues, formed either by ſetons, or by cauſtic, or, what M. Portal conſiders preferable, by means of moxa, the uſe of ſoap-pills, of a decoction of ſarſaparilla, of the juice of endive, together with a milk-diet, warm-bathing, and in ſome caſes the application of leeches to the anus.

Beſides the above ſpecies of rickets, M. Portal mentions two others. The one of theſe is, that which ariſes from caſtration at an early period of life, or by the ſhocking practice of maſturbation. It is unneceſſary to obſerve, that when the diſeaſe is produced by the former of theſe cauſes, it is incurable, The latter cauſe is with difficulty, he ſays, overcome, as the unfortunate victims of the practice alluded to, are ſo infatuatedly addicted to it, that no reaſoning can, in general, prevail upon them to relinquiſh it. Where, however, that can be accompliſhed, cold-bathing, the uſe of the peruvian bark, nouriſhing diet, change of air, and frictions of the ſpine and extremities, contribute greatly to reſtore health. The other ſpecies is common both to children and adults. It ariſes from the irregular contraction of the muſcles; and this is occaſioned, either by a ſpaſm or a paralyſis taking place in ſome of the muſcles. The ſpine is the part affected in ſuch caſes.

For the cure of this diſeaſe, warm bathing, and ſuch ſpecies of exerciſe as are calculated to ſtrengthen the weakened muſcles, together with friction, and in ſome caſes the uſe of bliſters or ſetons, are recommended.

IX.

S. Th. Soemmering *ueber das Organ der Seele.*

S. T. Soemmering *on the Organ of the Soul. With plates.* 4to. Koenigſberg, 1796.

See Archiv fuer die Phyſiologie von D. Joh. Chriſt. Reil, *Profeſſor in* Halle. *Vol.* I. *Part.* III. 8vo. Halle, 1796.

THE manner in which the different nerves terminate in the brain, is remarkably uniform. Except in evident monſters, there is hardly any ſtriking variation: P. Soemmering reckons 43 pairs of nerves; 12 proceeding from the brain; 30 from the ſpinal marrow; and 1 ſympathetic pair. We muſt not, ſays he, imagine, that the ſides of the ventricles are every where in contact; for in the horns of the lateral ventricles, and in the third and fourth ventricles, there is an evident ſpace, which is always filled with a fluid. P. Soemmering does not recollect ever having found them adhering, except in a ſmall degree, along the corpora ſtriata.

The terminations, or real commencements, of moſt if not of all the nerves, occupy determined and very conſtant ſituations on the ſides of the ventricles; or may be traced ſo far, that we may ſay they come in contact, at determined places, with the fluid contained in the ventricles.

The real terminations of the auditory nerves are obvious, without any other preparation, than ſimply opening the fourth ventricle. They appear on its lower part, as ſeveral medullary lines, which ſeem inlaid, and are eaſily diſtinguiſhed from the grey ſubſtance of the ventricle, by their milk-white colour. Thoſe of the right and left ſides are ſeparated by a furrow. Santorini found them evidently projecting above the ſurface of the ventricle, in the brain of a blind man, who had a very acute ſenſe of hearing. Here, then, the terminations of the auditory nerve and fluid, contained in the fourth ventricle, are in immediate contact; and in caſe the oſcillations, excited in the organs of hearing, be propagated farther than theſe ſolid terminations, they muſt be communicated to this fluid. If this be true, it is alſo probable, that the ſenſations of hearing ariſe alſo in this fluid; and that it is their ſenſorium commune.

It is well known, that the origins of the optic nerves, are in mutual contact with the fluid of the lateral venticles; and that the middle of the place of their decuſſation, is ſo with that of the third ventricle. It is, therefore, likewiſe probable, that the oſcillations of the optic nerves are communicated to the fluid of the lateral ventricles; and that the ſenſations of viſion originate in it, and have in it their ſenſorium commune.

Moſt of the mammalia have a thick, ſhort, and hollow pair of olfactory nerves; which are cloſe anteriorly, towards the cibrous part of the œthmoid bone; but poſteriorly, communicate openly, freely, and evidently, with the ventricles. The oſcillations, therefore, of the olfactory nerves, are propagated to the fluid of the ventricles. And ſome animals enjoy a more perfect ſenſe of ſmelling than man; perhaps, becauſe in adults the olfactory nerves cannot be ſo well traced as in thoſe animals, into the corpora ſtriata and ſides of the ventricles themſelves. But, in embryos of three, four, and five months, this nerve appears to be the thickeſt of all; is evidently hollow; ariſes like a crooked horn from the anterior lobe of the brain; and is obviouſly connected with the lateral ventricles.

Prof. Soemmering has alſo traced the third pair, almoſt to the ſides of the ventricles, through the dark ſubſtance; and Zinn even to the anterior commiſure. The fourth pair lies on the valve, or roof of the fourth ventricle; and their commencement may be traced, with a little attention, into its ſubſtance. By accident, Prof. Soemmering diſcovered, that the fifth pair may be followed almoſt to the lower ſide of the fourth ventricle; but has not yet been able to trace the ſixth pair, through the ſubſtance of the brain, to the ſides of the ventricles. Fibres of the facial nerve, or ſeventh pair of our author, may always, according to Malacarne, be traced to the fourth ventricle; and the pharyngeal, and nerves of the organs of ſpeech, may ſometimes be followed as far.

If the commencement of the par vagum, of the lingual and of the ſpinal nerves, could be traced as diſtinctly to the ſides of the ventricles, it is impoſſible that the idea of the fluid in the ventricles, being the ſenſorium commune, could have eſcaped phyſiologiſts. But, from what has been already noticed, it appears that the nerves of taſte, ſmell, viſion, and hearing, are expoſed to its action; and that the ſame takes place with regard to the nerves of touch, originating from the fifth pair, the gloſſo-pharyngeal nerves; thoſe belonging to the organs of voice, and the motions of the eyes. Mr. Soemmering thinks it, therefore, probable that the ſenſorium commune is in the fluid of the ventricles.

Hitherto, a ſolid part of the brain has been looked for, into which the terminations of all the nerves might be traced, but in vain. If the ſenſorium commune exiſt where all the nerves come together in the brain, it is in the ſides of the ventricles, where their real terminations approach, and are actually united by the fluid contained in them. But this opinion obliges our author to inquire, whether a fluid can be animated? and he endeavours to prove it by many obſervations and arguments.

The fluid of the ventricles, he alſo thinks, can permit a different motion to all the five ſenſes, at the ſame time without confuſion; for, according to Chaldni's experiments, each tone produces a peculiar form of vibration, even on the ſurface of water. Motion, indubitably, takes place in every nerve which excites ſenſation; and as it remains the ſame as long as the nerve remains the ſame, it is therefore propagated, unchanged, to its termination in the brain, and tranſmitted from it to the fluid of the ventricles, at which time it muſt undergo a change. Hearing is our juſteſt ſenſe; and no nerve is ſo immediately connected with the fluid of the ventricles, as the auditory nerve. The nerves of our fineſt ſenſes, hearing and ſeeing, are moſt directly in contact with this fluid; they terminate in the ventricles very diſtantly; and are differently expanded there. No animal has ſo capacious and ſo perfectly organized ventricles as man. In the other mammalia, they are leſs than in man; ſtill leſs in birds; leaſt in fiſhes; and abſolutely wanting in inſects.

There is a præternatural increaſe of fluid in the ventricles of rickety children; which is, perhaps, the cauſe of their uncommon acuteneſs of mind. If all the motions of the nerves concentrate in the fluid of the ventricles, in like manner, all motions coming from the brain will ariſe in the ſame fluid, from the mutual action which takes place between it and the terminations of the nerves on each other. The original effects produced on the fluid of the ventricles, may react on the ſolid parts of the brain; but this reaction muſt differ from the original action; therefore ideas and internal ſenſations muſt be weaker than external ſenſations. Perhaps original actions on the ſenſorium are followed by certain reactions, determined according to mechanical laws. When light, by means of the optic nerves, acts on the fluid of the ventricles, winking may be produced by a reaction taking place on the fibres of the third pair.

X.

Verſuch ueber die Schwimmblaſe der Fiſche. Eſſay on the Air-bladder of Fiſhes. By Gotthelf Fiſcher. 8vo. Leipſic, 1795. *With a Plate.*

Seu Archiv. fuer die Phyſiologie von D. I. C. Reil. *Vol.* II. *Part* I. 8vo. Halle, 1797.

THE more frequently natural hiſtorians attend merely to the external characters of things, eſpecially in organic bodies, without thinking of their nature, functions, and economy, every work which furniſhes us with additional information reſpecting the proper phyſics of animals, muſt be more welcome.

The preſent intereſting Eſſay, which induces us to hope much from the author's happy ſpirit of obſervation, is divided into three parts. In the firſt, the authors who have treated this ſubject are noticed; Needham, Redi, Preſton, Erxleben, Monro, Vicq d'Azyr, Block, Schneider, &c.

In the ſecond, he treats of the denominations, ſituation, and ſtructure of the air-bladder; and refers to the different figures of it which have been publiſhed. It is a white membránous bag, either ſingle or double, and united in the middle by a ſmall tube, connected with the œſophagus or ſtomach, filled with air, and extending along the ſpine far into the abdomen. Near the diaphragm, it adheres to the ſecond vertebra, and covers the kidneys. Fourcroy found it, in the carp, to contain azotic and oxygen gaſes; which our author confirms from his own experiments. In other reſpects, the air-bladder is ſubject to many varieties: in ſome fiſhes, it is entirely wanting; in ſome, it is ſingle; in others, double; and in that caſe the upper and under portions are connected by a canal, immediately above which the pneumatic duct enters, accompanied by many conſiderable veſſels, The cobitis foſſilis has a bony ſack, lined with membrane, which adheres to the third and fourth ribs. In the porcupine fiſh, it is cellular, like the lungs of frogs; reſembling ſomewhat ſmall bladders, connected in the form of a bunch of grapes. The ſame remarkable ſtructure occurs in the globe-fiſh.

The pneumatic duct is that canal which unites the air-bladder with the œſophagus. When the bladder is ſingle, it is ſhort and wide; ariſes from its lower ſide; and opens between the longitudinal fibres of the gullet, which, by their natural action, can ſhut it. Where the air-bladder is double, it is longer; ariſes near the canal, uniting its diviſions; paſſes, accompanied by a number of veſſels, over its upper part; loſes itſelf, with the veſſels of the liver, behind its lower tendinous membrane; and at laſt arrives at the place where it unites with the gullet, forming a ſmall ſwelling. Some anatomiſts have ſought valves here; but our author thinks it a kind of ſphincter. He next quotes ſeveral engravings of the air-bladder, publiſhed by different authors; and adds a new and accurate plate of thoſe of the pike and tench, with their pneumatic ducts, and the vertebra to which they adhere. Laſtly, he has given a liſt of thoſe fiſhes which have, and of thoſe which have not an air-bladder.

The third and laſt part, treats of the uſe of this bladder. Some have thought to diſcover this, by means of the air-pump. The fiſhes died in a rarified atmoſphere; and, on opening them, the air-bladder was found empty, though not ruptured. But theſe experiments only teach us, that fiſhes cannot live in water exhauſted of air; and confirm the conjecture, that they decompoſe air, as well as water. Others pierced through the ſcales, fleſh, and air-bladder, between the third and fourth ribs; on which the fiſh ſunk, inverted, to the bottom. From this they concluded, that the air bladder was of uſe to the fiſh in ſwimming; but never once thought of the injury done by ſo conſiderable a wound, or of the influence of the admiſſion of water into the cavity of the abdomen, on the health of the animal. That the air-bladder aſſiſts the fiſh in ſwimming, our author allows;

but does not think this its eſſential uſe. For, ſays he, if it were only ſubſervient to the purpoſe of ſwimming, why is it ſupplied with ſo many blood-veſſels? Why does it contain azotic gas, and not the lighter hydrogen gas? By what power is it ſo quickly emptied, and again filled? for it has no muſcular fibres, and the abdominal muſcles do not act on it. Erxleben thought its bulk was changed by the rarification and compreſſion of its air. Needham imagined that the air produced in the blood was depoſited in it, and afterwards emptied into the ſtomach, where it aſſiſted digeſtion. Vicq d'Azyr ſuppoſed it to be a kind of ſubſidiary ſtomach, where the finer parts of the food were received and abſorbed. All theſe, and ſome other opinions, are here ſucceſsfully combated by our author.

Mr Fiſcher himſelf imagines, that the air-bladder, beſides favouring the motion of the fiſh in its element, is an organ which ſeparates oxygen from the atmoſpheric air contained in the water, in the ſame way as the gills ſeparate it from the water, and completes the formation of the blood, which is only imperfectly begun in the gills, as imperfect lungs. He thinks that the reſpiration of fiſhes is more complicated than is uſually believed; and that its object is not merely the decompoſition of the water, but alſo of the atmoſpheric air contained in it. This explains the riſing of fiſhes to the ſurface of the water, and their catching at air; which ſometimes, eſpecially in ſultry weather, cauſes them to make conſiderable leaps. The ſurface with which the blood is expoſed in the gills, is not ſufficient for its abſorbing the neceſſary quantity of oxygen. Some place, therefore, becomes neceſſary, in which the blood, or ſome particular portion of it, may be again expoſed to its action. This is the air-bladder, where the air contained in the water is decompoſed; which is proved by the azotic and carbonic acid gaſes left in it. Theſe the fiſh can empty by the pneumatic duct, as voluntarily as it can again fill it with atmoſpheric air.

This opinion of the uſe of the air-bladder, is, he thinks, ſupported by its whole ſtructure, the nature of its vaſcular coat, its more vivid colour, and, laſtly, by its cellular appearance, reſembling the lungs of frogs, in the porcupine and globe fiſhes.

XI.

Programma del Modo d'agire ſul Corpo Umano, per Mezzo di Frizioni fatte con Saliva ed altri umori Animalizzati, ecolle varie Soſtanze, che all'ordinario ſi ſomminiſtrano internamente.

Diſcourſe on the Mode of Acting on the Human Body, by Means of Frictions made with Saliva and other Animal Fluids, and the various Subſtances commonly given internally. Recited in the Hall of the Univerſity of Pavia. By Citizen Valerian Lewis Brera, M. D. *Public Prof. Extraordinary of the Theory and Practice of Medicine; Clinical Lecturer, and Chief Surgeon to the National Legion of Pavia, &c. &c. Third Edition.* 8vo. Pavia, 1ſt of the Ciſ. Rep. (1797.)

THIS new method of exhibiting remedies, firſt propoſed by Dr Chiarenti of Florence, and afterwards extended by

Dr Brera of Pavia, has excited too much attention in Italy and in France, to permit us to leave it unnoticed, however little we may be diſpoſed to coincide with its ſupporters in their opoinion of its vaſt utility, and expectations of the benefits to be derived from it.

Dr Chiarenti, a zealous follower of Spallanzani, recommended the gaſtric juice as an excellent remedy in diſeaſes originating from debility of the ſtomach. In giving opium at the ſame time, he found that it often occaſioned much uneaſineſs and vomiting. This he aſcribed to its remaining on the ſtomach indigeſted, from the vitiated gaſtric fluid not acting on it; and led him to reflect on the reaſons why opium, adminiſtered externally, had ſo little action. In conformity with his theory, he explained this, by ſuppoſing that the opium had not undergone that change which is neceſſary for its action; and he determined to bring his opinion to the teſt of experiment. An occaſion ſoon offered. A woman, afflicted with violent pains, refuſing to take opium by the mouth, was a fit ſubject for commencing his trials. He mixed three grains of pure opium with two ſcruples of the gaſtric juice of a crow. It ſoon emitted a ſtrong and penetrating ſmell, which diminiſhed gradually. In half an hour, the opium was perfectly diſſolved; but it was permitted to remain twenty-four hours. It was then mixed with ſimple ointment, and rubbed on the backs of the feet. In an hour, the pains were entirely gone; and, never returning, the woman remained cured.

Similar ſucceſsful trials being made with opium, ſquills, and foxglove, our author was induced to repeat them, in the clinical ward under his care. By the difficulty of procuring a ſufficient quantity of the gaſtric fluid, to enable him to carry his experiments the length he wiſhed, he was led, by analogy, to ſubſtitute in its place ſaliva; and the reſult anſwered his expectations.

Suſpecting that neither the gaſtric juice nor ſaliva contributed, by any ſpecific power, to the action of the remedy, he proceeded to make ſome trials with ſquills and opium, incorporated with volatile liniment.

ment. But the only effects produced were owing to the ſtimulating nature of the application.

Dr Ballerini, of Mantua, cured one dropſy with frictions of ſix grains of ſquills, diſſolved in gaſtric juice, made every ſecond evening; and another, with a ſcruple of ſquills, diſſolved in a drachm of ſaliva, rubbed in at three times during the courſe of the day. The aſſiſtants who made the frictions had likewiſe an increaſed flow of urine.

Prof. Giulio, of Turin, has derived ſo much advantage from this method of adminiſtering diuretics, that he is preparing a treatiſe on the ſubject.

Prof. Brera next relates his own ſucceſsful trials, with ſaliva and opium in chloroſis; with ſquills, digitalis purpurea, and epiglottis, corroſive ſublimate, aconite, and tartar emetic. And to theſe he ſubjoins the teſtimonies of Dr Benvenuti at Turin, Drs Picoſſi and Locatelli in Milan, and Dr Bottelli. From all theſe obſervations, he is led to conclude, that every animaliſed fluid is fitted by nature to render remedies capable of being abſorbed.

XII.

Sull 'Uſo Eſterno dell Oppio. Letter on the External Uſe of Opium, to Dr L. Frank, *from* Vincenzio Chiarugi, M. D. *and Phyſician to the Hoſpital of St Boniface, at Florence.* 8vo. Florence, 1797.

IN this letter, the obſervations contained in the preceding article are partly contradicted, and partly confirmed. The author, who is well known, from his very elaborate work on mental derangement, is chief phyſician to the great Hoſpital of St Boniface, which is appropriated entirely to maniacs.

In treating their diſeaſes, Dr Chiarenti's diſcovery promiſed to be of great imporatnce; and our author immediately reſolved to carry it into practice. He was not, however, ignorant of the effects already obſerved by many practitioners, from the external exhibition of this drug, and was perſuaded, that its action on the nervous ſyſtem might be obtained, though not introduced into the ſtomach, without being diſſolved in the gaſtric fluid, from the mere emanation of its volatile aroma. He, therefore, reſolved to try it in the form of a ſimple ointment, made by incorporating a drachm of finely powdered opium with a pound of axunge. An ounce of this ointment, containing ſix grains of opium, procured ſleep to a reſtleſs boy, who had been an idiot from his birth. Its effects were ſtill more ſurpriſing on a peaſant, in the height of a paroxyſm of the moſt furious mania. Theſe frictions always rendered him calm, and ſometimes threw him into a ſtate of lethargic ſtupor. In this caſe, a complete cure was at laſt effected.

It was exhibited in the ſame manner to twelve other perſons, affected with mental or reactive madneſs; and, from his obſervations on them, he concludes, that although the ſleep produced was not always proportionate, either in intenſity or duration, to the doſe of the opium; yet a ſtate of calmneſs conſtantly ſucceeded, ſooner or later; and five of them have been cured, without the uſe of any other remedy. Certain of the efficacy of the opiate ointment, he tried frictions of laudanum, diluted with a little alcohol; and found them equally ſucceſsful.

Theſe experiments being all made on patients who were tied, there could not be any ſuſpicion that the opium was ſwallowed. Dr Chiarugi thinks, that they fully eſtabliſh the fact, that opium, however applied to the ſkin, penetrates it, and exerts its action diffuſively on the nervous ſyſtem, without any neceſſity for its being mixed with gaſtric juice, provided it be ſufficiently divided or diluted.

Although, therefore, our author does not agree with Dr Chiarenti in all his opinions, he confeſſes the utmoſt obligations to him, for having led him to a practice, from which he has derived much benefit, in a diſeaſe in which it is ſo difficult to exhibit medicines internally.

XIII.

Analyſi Chimica della China Gialla, &c. i. e. Chemical Analyſis of the Yellow Peruvian Bark, lately introduced into Practice. With Obſervations on its Medical Uſe, and on that of the common Peruvian Bark. By Francis Marabelli, *Apothecary to the Hoſpital of Pavia, Public Repeater of Chemiſtry, Mat. Med. and Pharm. in the Univerſity, &c. &c.* 8vo. Pavia, 1795.

THE advantage and neceſſity of being acquainted with the properties and principal conſtituents of the ſubſtances employed in medicine, are ſufficiently obvious. To increaſe this kind of knowledge, our author has exerted himſelf ſucceſsfully. In the preſent work, he has emulated the moſt celebrated modern chemiſts; and has deſcribed, with peculiar perſpicuity, a ſeries of accurate experiments, of whoſe concluſions we ſhall attempt to give a general idea.

A pound of the yellow bark, in powder, was infuſed in five pounds of diſtilled water, at a temperature of about 60° of Fahrenheit. The ſame proceſs being repeated five times, the ſixth infuſion had little taſte or colour. The whole thirty pounds of fluid were mixed and evaporated. When they were reduced to about one third, red flakes began to be ſeparated; which continued to float looſely, until only about five ounces of fluid remaining, it was permitted to cool, and then filtered. The ſtrained liquor was ſomewhat thick, of a deep red colour, and an auſtere, ſalt, and ſlightly bitter taſte. By means of chemical agents, it was found to contain of

	Scr.	*gr.*
Mucus or Gum, - -	10	6
Extractive-mucous matter,	13	6
Extractive-reſinous matter,	0	16
Muriat of Lime, - -	1	16
——— Magneſia, -	0	10
Citric Acid, - - -		12
	26	18

N. B. There are twenty-four grains in a ſcruple, according to the weights here employed.

The red matter, collected on the ſtrainer, had a bitter, ſlightly ſalt, and auſtere taſte. At firſt it crackled under the teeth, but afterwards melted in the mouth. It conſiſted of

	Scr.	*gr.*
Extractive-reſinous matter,	4	19
An inert ſubſtance, -	1	$5\frac{34}{49}$
Gluten, - - -	0	$12\frac{23}{49}$
Sulphat of Potaſh, -	1	10
Nitrat of Potaſh, -	1	22
Muriat of Lime, -	0	6
Gallic Acid, - -	0	00
	10	$3\frac{16}{49}$

The whole quantity of matter, therefore, extracted from one pound of yellow bark, by the action of cold water, was about three ounces and twenty-one grains. The extractive-reſinous matter was remarkable for its peculiarly elegant colour, and for the property of tinging deeply the very great quantity of water neceſſary for its ſolution.

lution. It was more ſoluble in alcohol, and with the aſſiſtance of heat, alcohol, diluted with water, diſſolved it abundantly. The inert ſubſtance did not exiſt originally in the bark; but, as our author has clearly proved, was produced by the oxygenation of part of the extractive reſinous matter, during the evaporation.

The reſt of the ſame pound of bark, from which the infuſions were made, was now repeatedly boiled. The fifth decoction acquired no taſte, and very little colour. In all, they amounted to forty pounds, which, were reduced, by evaporation, to about eight ounces; and, when cold, were filtrated.

The flaky matter, which was collected on the ſtrainer, was of a browniſh red; had a bitter taſte, did not crackle under the teeth, but melted readily in the mouth. Its conſtituents were,

	Scr.	*gr.*
Extractive-mucous matter,	5	6
An inert ſubſtance, -	1	$22\frac{26}{49}$
Extractive-reſinous matter,	0	19
Gluten, - - -	0	$9\frac{15}{49}$

Nitrat

	Scr.	gr.
Nitrat of Potaſh, - -	1	3
Gallic Acid, - - -	0	0
	9	$21\frac{41}{49}$

Theſe flakes, therefore, differed from thoſe obtained from the cold infuſions, in conſiſting principally of extractive-mucous matter; and in the colouring matter, which was not ſo elegant as in the others, being combined with it, not with the extractive-reſinous matter. The quantity of the inert ſubſtance was naturally greater, as its ſource had been longer expoſed to the action of heat and air.

The liquor which paſſed through the filter, contained of

	Scr.	gr.
Mucus or Gum, -	9	2
Extractive-mucous matter,	1	2
Extractive-reſinous matter,	5	22
Reſin, - - -	7	10
Nitrat of Potaſh, - -	2	12
Muriat of Lime, -	2	2
Muriat of Magneſia, -	0	9
Gallic Acid, - -	0	0
	28	11

Alcohol extracted from the pound of yellow bark, already ſubjected to all theſe operations, twenty-ſeven grains of reſin.

Its conſtituents were therefore,

	Oun.	ſcr.	gr.
Mucus or Gum, -	0	19	8
Extractive-mucous matter,	0	19	14
Extractive-reſinous matter,	0	12	4
Inert matter, - -	0	3	$4\frac{28}{49}$
Reſin, - - -	0	8	13
Nitrat of Potaſh, -	0	5	13
Sulphat of Potaſh, -	0	1	10
Muriat of Lime, -	0	4	0
——— of Magneſia, -	0	0	19
Citric Acid, - -	0	0	12
Gallic Acid, -	0	0	0
	3	3	$23\frac{8}{49}$
Woody Fibres, - -	7	9	0
	10	12	$23\frac{8}{49}$

The quantity of gallic acid is never mentioned, becauſe our author was unable to deviſe any means by which he could aſcertain it. Its weight is, however, comprehended in that of the two extractive-matters with which it was united.

Notwithſtanding every poſſible precaution, there was therefore a loſs of ʒ1: 11: 0$\frac{41}{49}$: probably owing to the evaporation of moiſture.

By incineration, ſome carbonat of lime, with a very ſmall portion of carbonat of magneſia, and of potaſh, and of ſulphat of potaſh, were obtained.

From another pound of this bark, by repeated decoctions with water and ſubſequent evaporation, our author got five ounces two drachms and a half of extract, including the water neceſſary for its conſiſtence: it was, however, leſs bitter than the bark itſelf. Hence that principle upon which the bitterneſs depended, muſt have been altered, probably from the abſorption of oxygen. From the phenomena which occured during the proceſs, the following practical obſervations reſult. The decoctions ſhould be made in cloſe veſſels, and as ſpeedily as poſſible. The extract, made by means of water, does not contain the active principles of the bark condenſed into a ſmaller bulk; but is an expenſive and feeble preparation. The ſediment which precipitates itſelf from a well-ſaturated decoction, conſiſts of reſinous extractive-matter, united to a conſiderable quantity of gallic acid; which are the moſt active parts of the bark. It is therefore the very ſubſtance upon which the activity of the decoction depends.

Alcohol extracted from the bark, not merely the reſin, but likewiſe the extractive-reſinous matter, gallic and citric acids, ſome gluten, the muriat of lime and magneſia, and a little ſulphat of potaſh. A ſpirituous vehicle may be therefore occaſionally uſed, as it is capable of retaining a much larger proportion of the active conſtituents of the bark in ſolution; and as it reſiſts the abſorption of oxygen, we may by its evaporation prepare an extract of real efficacy. It is likewiſe evident, that in order to obtain the pure reſin from any tincture, it muſt be ſeparated by the affuſion of water.

After having given the analyſis of this bark, and an account of its diſtillation with nitric acid, our author recapitulates ſome practical corollaries for its exhibition. The decoctions ſhould be made quickly, and in cloſe veſſels. Previous to their exhibition, they ſhould be well ſhaken, that the depoſition may be diffuſed in the fluid. They ſhould be kept in cloſed veſſels. No preparation of iron ſhould be added to them; while, on the contrary, bark ſeems to facilitate and accelerate the action of iron, when both are exhibited in ſubſtance. The epidermis ſhould be ſeparated from this bark, before uſing it in any way. After being powdered, it gradually loſes its activity by being kept. Whenever the ſtomach will bear it, it is beſt exhibited in powder; and ſmall reiterated doſes are equal in effect to larger ones given more ſeldom.

Our choice of the different preparations, muſt depend on circumſtances. In decoction, a larger quantity of the active conſtituents may be given; in tincture a larger proportion in leſs bulk, where a greater ſtimulus is required; and in cold infuſion, it may be exhibited to very delicate and irritable ſtomachs. The extract made by means of water, our author condemns as an uſeleſs preparation, and would ſubſtitute one made from the tincture. To leſſen its expence, he adviſes it to be made with very weak ſpirit, of which a great part might be regained by diſtilling it off, in place of evaporating it in the open air.

His analyſis has alſo ſuggeſted to him the idea of preparing a compound ſubſtitute for the extract of bark, by a proper combination of bitter and aſtringent extracts. He recommends experiments with this view; and hopes, that by mixing the ingredients in various proportions, by uſing various bitter and aſtringent ſubſtances, which might contain theſe qualities in a different form, and by combining ſeveral of them together, a ſubſtance may be at laſt obtained, which clinical experiments will prove to contain thoſe qualities hitherto looked for in vain in the extract of bark, at a ſmaller price, and not liable to adulteration.

Our author concludes his treatiſe with a very few obſervations on the medical effects of the yellow bark. He has found it conſtantly efficacious in intermittents, and often in a leſs doſe than the common bark. It alſo acted more ſpeedily and conſtantly in ſuppreſſion of menſes and cachectical complaints, in morbid ſenſibility and irritability of females, and ſeemed to mitigate the ſymptoms of hypochondriaſis.

XIV.

De Zea Mays, Analytica Diſquiſitio, Auctore Franciſco Marabelli. 8vo. Paviæ, 1793.

THIS likewiſe is an intereſting work, and claims our attention, as giving an excellent analyſis of an article which conſtitutes almoſt the ſole food of entire nations. Although our author's experiments be not exactly ſuch as we believe he would now inſtitute with the ſame view, yet they have furniſhed him with important reſults.

The maize contains a ſaccharine matter of different degrees of purity, from which alcohol, the oxalic and acetous acids, may be obtained; a vegetable amylaceous ſubſtance, a glutinous ſubſtance; muriat and nitrat of magneſia; carbonats of potaſh, lime, and magneſia; and iron.

Theſe ſubſtances exiſt in different proportions in different parts of the plant; but the ſaccharine matter, ſalts, and earths, are found in all of them.

Beſides theſe, the ſeeds, in every degree of maturity, contain the amylaceous matter and animal gluten.

The unripe, green ſeeds, ſtill abounding with a milky fluid, differ from the ripe and dried ſeeds in furniſhing a conſiderable quantity of juice, which is coagulable both by fire and different chemical agents, and reſembles milk not a little; and, in containing another ſubſtance, very ſimilar to the white of egg, coagulated by boiling water. This, in the progreſs of vegetation, ſeems to be combined in a new way, changed or deſtroyed; as it is not found in the ripe ſeeds, though they likewiſe contain an animaleſcent ſubſtance. Laſtly, They differ, becauſe the amyla of the unripe is whiter than that of the ripe ſeeds, and is very like that of wheat.

The ammonia, or its carbonat, obtained from the reſiduum of the expreſſed juice in a ſtate of putrefaction, ariſes from the potaſh changed by putrefaction; ſince the quantity of carbonat of potaſh, now got by incineration, is much ſmaller than that afforded by the ſame reſiduum before putrefaction commences, when there is not a veſtige of ammonia. Our author is therefore inclined to believe, that the fixed alkalies may be modified and changed into ammonia by fire or putrefaction, with the addition of an inflammable ſubſtance; eſpecially from having obſerved this change in ſome experiments, made with carbonat of ſoda, which he promiſes to publiſh on ſome other occaſion.

The taſte, ſmell, and yellow colour, of the meal and bread prepared from the ripe ſeeds, depend entirely on the animal gluten, diſcovered by our author in the ſeeds.

The flour of maize differs from that of wheat, in containing a larger quantity of ſaccharine matter, and leſs ſtarch, and in the gluten of the maize being mixed with a reſinous colouring matter, and not poſſeſſing the elaſticity or whiteneſs of that of wheat.

Laſtly, Although ſome parts of the maize produce a ſugar, ſomewhat purer than the ſtalks, theſe, however, produce a greater quantity; ſo that, except the ſugar-cane, there is no vegetable to be compared with it, either for quantity or quality. This has ſuggeſted to our author, the idea that this plant might be advantageouſly cultivated for the ſake of this ſugar; and with that view, it ought to be cut down while full of juice, before it is ripe; for thus there might be two crops from the ſame fields in one ſeaſon. And although there would be no corn produced fit for uſe; yet the leaves would ſerve for the feeding of cattle, and the amylaceous matter for fowls and domeſtic animals.

XV.

Eſame dell'Orina d'un Itterico, e Ricerche ſu di Alcuni Latterizii Sedimenti trovati in altre Morboſe Orine Umane.

Examination of the Urine of a Man afflicted with the Jaundice, and of the Lateritious Sediments of ſome other Morbid Human Urines. Made at the requeſt of Prof. J. P. Frank. *By* Francis Marabelli. 8vo.

FROM the numerous experiments which are here detailed, Mr Marabelli finds that the characters which generally diſtinguiſh the urine of a perſon labouring under jaundice, from healthy urine, are its particular colour, and intenſely bitter taſte; the production of a pellicle on the ſurface, after being permitted to reſt ſome time; its becoming frothy on being ſhaken; its property of giving a yellow tinct to linen, and a yellowiſh green to abſorbent earths immerſed in it; and, laſtly, its acquiring a fine green colour from the addition of nitric acid.

All theſe characters are owing to a foreign, colouring matter, of a reſinous nature, which being diſſolved by means of the different ſalts, renders the urine ſaponaceous, ſo as to become frothy by agitation. It ſeems to be very looſely combined, as by the mere immerſion of ſome ſubſtances, it is ſeparated, and the urine reduced to a perfectly natural ſtate.

By means of ſimple evaporation and putrefaction, it loſes its bitter taſte; which ſeems, therefore, to be owing to ſome very volatile principle, (probably of the nature of eſſential oils), united to the reſin.

From all his experiments, our author is induced to believe, that the common opinion, that theſe properties are owing to bile contained in the urine, is unfounded. At leaſt, it muſt have undergone ſome conſiderable change or decompoſition; for real bile does not loſe its bitter taſte by becoming putrid; and it is rendered more intenſe by evaporation. Beſides, bile, diſſolved in alcohol, is not decompoſed by the affuſion of water; while a ſpirituous ſolution of the colouring matter of jaundiced urine is rendered milky by the precipitation of the reſin.

The lateritious ſediments appear to conſiſt of an animal matter and phoſphat of lime.

XVI.

Memoria ſui Principi e ſulle Differenze dell Orina in due ſpecie di Diabete, confrontata Colla Naturale.

Eſſay on the Principles and Differences of the Urine in two ſpecies of Diabetes, compared with Natural Urine. Addreſſed to J. P. Frank. *By* Francis Marabelli. 8vo.

THE inveſtigations which form the ſubject of this Eſſay, were undertaken in 1788, like moſt of our author's reſearches, by the deſire of J. P. Frank, then Clinical Profeſſor in Pavia. From an accurate analyſis of the urine of two patients, afflicted with diabetes inſipidus, he infers, that the urine in this diſeaſe, although in different ſubjects it have ſome diverſity, differs very little in its principles from healthy urine;

conſiſting of a large proportion of water, a very little uncombined phoſphoric acid, an animal ſubſtance, ſometimes ſulphat of potaſh, and always muriat of ſoda, phoſphat of ſoda and ammonia, ſulphat of ſoda, and a ſaponaceous extractive matter, without containing any ſaccharine matter, or ſalts with earthy baſes.

His experiments on the urine, in diabetes melitus, coincide with thoſe of Dobſon, Black, and others. We ſhall, however, notice their reſults, as being a ſubject which at preſent attracts much attention.

Sweet diabetic urine ſeems to conſiſt of much water, a little uncombined phoſphoric acid; a large quantity of ſugar, by means of which, the urine can furniſh oxalic acid, alcohol, and acetous acid; a ſmall quantity of mucus, a little muriat of ſoda and phoſphat of ammonia; a few particles of iron; a ſmall quantity of extractive ſaponaceous matter, and in ſome caſes, where the ſugar is deficient, the quantity of mucus is proportionably increaſed.

It differs from natural urine, in remaining

ing always clear, without ſediment; in being ſweet; in undergoing the vinous fermentation; in furniſhing a white precipitate on the addition of a ſolution of nitrat of mercury, or at leaſt one leſs coloured than the phoſphat of mercury of other urine; in acquiring, by evaporation, the conſiſtence of a ſyrup, which is without ſmell, ſcarcely coloured, extremely ſweet, and, on cooling, neither depoſits cryſtals, nor any thing elſe, but becomes grumous like honey: while, by the ſame proceſs, common urine acquires a black colour; gives out a fœtid ſmell, and on cooling, depoſits much ſaline and earthy matter; in its extract being viſcid and thready, burning with the ſmell of ſugar, and giving to alcohol a yellow colour: while that of other urine is not viſcid, on burning emits a urinous ſmell, and tinges alcohol black; and in containing a large quantity of ſugar; or, inſtead of it, much mucus, with fewer ſalts, and in leſs quantity than other urine; which is entirely void of ſugar, and probably alſo of mucus.

The chief diſtinctions between ſweet and inſipid diabetic urine, are owing entirely to the ſugar, or the mucous ſubſtance, which ſeems to contribute to its formation. The preſence, therefore, of one or other of theſe matters, conſtitutes the diſtinguiſhing character of the urine in diabetes melitus.

XVII.

Richerche intorno alla Natura dia lcune Materie Animale alterate da Malattie, e ſpezialmente di alcune ſomiglante al Latte.

Inquiry into the Nature of ſome Animal matters altered by Diſeaſe; and eſpecially ſome reſembling Milk. Addreſſed to J. P. Frank. *By* Francis Marabelli.

THE object of this Inquiry is to decide the queſtion, Whether the milky fluid found in the abdominal and other cavities of women who have died of puerperal diſeaſes, be really milk, depoſited there by metaſtaſis, or not? Marabelli collected this fluid in conſiderable quantity from the abdomen and thorax of a woman who had died of puerperal fever. It was of a moderate thickneſs, not tranſparent, of a cinereous rather than milky colour; without ſmell, of a ſalt, but not unpleaſant taſte. After he had freed it from the coagula which it contained, on ſtanding ſome time, it ſeparated a ſubtile whitiſh matter; which, however, remained ſuſpended, and was obliged to be ſeparated by the filter.

The coagula, on burning, emitted a fœtid ammoniacal ſmell; their aſhes were found to be phoſphat of lime. They were ſoluble in the mineral acids. From theſe ſolutions, alkalies cauſed a precipitate; which, on being added to exceſs, they again diſſolved.

The liquor from which theſe coagula were taken, did not affect the vegetable colours, or emit any ſmell on the addition of lime. The carbonated alkalies cauſed no change in it; but potaſh, ſoda, and ammonia, converted it into a jelly, from which a ſmall quantity of liquid ſeparated itſelf. The nitrous acid formed with it immediately, a roſe-coloured coagulum. The other acids, and alcohol, formed with it more ſlowly, and in leſs quantity, an aſh-coloured coagulum. It was likewiſe coagulated by the ſulphat of iron and nitrat of mercury, which were alſo in part decompoſed. Expoſed to a heat of 72° Fahren. it ſoon grew putrid, without ſhewing any previous ſigns of aceſcency. Laſtly, Mixed with ſolutions of the carbonats of potaſh or ſoda, by the action of fire, an albuminous matter was ſeparated.

By filtration, it became of a diaphanous grey colour; and was ſimilarly affected by the ſame agents. The matter which remained on the filter poſſeſſed the ſame characters with the coagula.

In order to inſtitute the compariſon more accurately, our author ſubjected ſome milk, drawn from the breaſt of a young healthy nurſe, to the ſame analyſis. It ſeemed more fluid, leſs white, and ſweeter, than ſkimmed cow-milk. At 72° Fahr. it very ſoon grew ſour; and emitted the ſmell of ammonia, on rubbing it with lime. It putrified ſlowly, and ſeparated only a few very fine white flakes. The alkalies formed in it, very ſlowly, a few white filaments; their carbonats tinged it ſlightly yellow; the acids and alcohol, after a conſiderable time, only produced a thick white cloud; nor was it coagulated by the metallic ſolutions mentioned above. The few flakes, afforded during the putrefactive fermentation, were diſſolved more readily by the pure alkalies, than by their carbonats. They were ſoluble in the acids; and a ſuperabundance of the alkalies, rediſſolved the precipitate they produced in theſe ſolutions. On burning, they gave out an ammoniacal ſmell; and their aſhes were evidently ſalt.

To render this comparative analyſis more complete, our author proceeded to examine ſome other animal fluids. And he found, that a fluid matter, which he collected in large quantities from the thorax and abdomen of two men who had died of violent peripneumony, agreed in every reſpect with that collected from the puerperal women.

A puriform fluid, diſcharged from the umbilicus of an aſcitic woman, was thick like mucilage; of a milky colour; fœtid, ſalt, and nauſeous. Expoſed to the heat of boiling water, it evaporated into two diſtinct parts, like cow-milk. Evaporated almoſt to dryneſs, it reſembled cheeſe beginning to ſpoil; on burning, it ſmelled like cheeſe, and left a black charcoal, containing much muriat of ſoda.

On being poured into diſtilled water, it fell quickly to the bottom, except a ſmall part which ſeemed diſpoſed to ſwim. By agitation, they formed a milky fluid; which, after ſtanding ſeveral hours, ſeparated into a pellucid liquid, with a light ſediment like albumen.

It coloured the ſyrup of violets green; which, our author ſays, he has always obſerved pus to do. A ſolution of carbonat of potaſh rendered it rather more fluid and ſweet; but, even with the aſſiſtance of heat, did not diſſolve it, after being ſomewhat thickened by evaporation. On pouring into this mixture nitric acid, it aſſumed a fine roſe-colour, an effect peculiar to this acid. It ſwam above a ſolution of potaſh; and, when mixed with it, by long agitation, it formed a fluid like the mucilage of gum-arabic; frothy, and unalterable by water, but depoſiting a few flakes on the addition of the acids.

The ſulphuric acid, being poured copiouſly upon it, grew warm; and changed its colour ſucceſſively from brown to green, and laſtly to black, which it retained. From this ſolution, water, and a ſolution of carbonat of potaſh, ſeparated a number of light white flakes; ſolution of the carbonats of potaſh, and ammonia, ſeparated brown ones, which afterwards became red.

Diluted nitric acid did not diſſolve it, without the aſſiſtance of heat; but then it formed a tranſparent, yellow, very fluid ſolution; from which neither water, nor alkaline ſolutions, precipitated any thing. But, on its evaporation, a ſubſtance adhered firmly to the ſides of the veſſel; reſembling yellow wax in its external characters and manner of burning, but differing in being entirely ſoluble in alcohol, and in water rendering the ſolution milky.

From theſe experiments, it appears evidently, that the fluid depoſited in the viſceral cavities of puerperal women, is not milk, but rather approaches to pus; at leaſt, according to the criterion of Dr Graſmeyer: which conſiſts in rubbing the matter to be examined with an equal quantity of lukewarm water, and then with the ſame quantity of a ſaturated ſolution of potaſh. If any purulent matter be preſent, Dr Graſmeyer ſays, that in the courſe of a few hours, a gelatinous ſubſtance will be formed; which is not the caſe with pure mucus. Our author found the firſt concluſion always confirmed; but confeſſes that the ſecond is not accurate; for often, when there could be no ſuſpicion of pus, the coagulation took place.

XVIII.

Lettera concernente l'Eſame dell' Acqua cavata colla Paracenteſi, da un' Idropico della Clinica di Pavia.

A Letter on the Examination of the Fluid, obtained by Tapping, from the Abdomen of a Dropſical Patient in the Clinical Ward of Pavia. To Prof. J. P. Frank. *From* Francis Marabelli.

THIS fluid retained its tranſparency, and lemon colour, ſeveral hours after its evacuation. It had a ſalt taſte, and no ſmell. Its ſpecific gravity was 1013. With vegetable blues, it ſhewed the preſence of an uncombined alkali. It was not changed by the addition of the alkalies; but coagulated by a ſufficient quantity of alcohol, or the acids. By boiling, it became turbid, white, and ſeparated a ſubſtance like coagulated white of egg. Twenty-eight ounces of liquid furniſhed two ounces and a half of this coagulum. By drying, it became light, hard, and diaphanous, of the colour of green bottle-glaſs. Liquid potaſh diſſolved it with a moderate heat; from which ſolution, the acids precipitated a white matter, and extricated ſulphurated hydrogen gas. By diſtillation with a large quantity of nitric acid, it did not form oxalic acid; but a very denſe, viſcid ſubſtance, of a green colour, verging towards black; of a very bitter taſte, tinging linen yellow; ſoluble in alcohol and in water; in ſhort, very ſimilar to bile. During the proceſs, there were ſeparated ſmall quantities of nitrat of ſoda and ſulphat of lime; and, with the nitrous acid in the recipient, there was mixed a little muriatic acid. The liquid from which this coagulum had been ſeparated by boiling, was found, on evaporation, ſtill to contain ſome albuminous matter and carbonat of ſoda.

From theſe experiments Mr Marabelli concludes, that the fluid examined by him conſiſted of water, coagulable animal matter, carbonat and muriat of ſoda, ſulphat of lime, and, probably, a ſaponaceous ſubſtance, reſembling bile, or ſome of its principles.

XIX.

Obſervations on the Zoonomia of Dr Darwin. *By* Thomas Brown, Eſq. 8vo. Edinburgh, 1798.

THE ingenious author of this work attempts to review the leading principles of the Zoonomia; for which purpoſe, he has, in ſome inſtances, departed from Dr Darwin's arrangement; and in others, he has condenſed ſeveral ſections into one. He has avoided, too, any particular ſtrictures on the noſology of the ſecond volume; one remark being, in his opinion, applicable to the whole, namely, That it is inconſiſtent with the fundamental principles of the theory by which one faculty of the ſenſorium cannot increaſe or decreaſe, without a correſponding increaſe or decreaſe of the others. Madneſs and fever, however, as being peculiarly intereſting, and conſidered by Dr Darwin in a new light, he has made the ſubject of two ſections, and under the title of Miſcellaneous Obſervations, he has comprehended a few remarks, which were not of ſufficient conſequence to form ſeparate ſections.

The 1ſt ſection relates to the ſenſorial power or ſpirit of animation, which Dr Darwin ſuppoſes to be a ſubtile fluid, that produces the phenomena of life, by its own motions, and by the motions which theſe excite in the fibrous parts of the ſyſtem. Our author endeavours to ſhew, firſt, That, according to the Doctor's principles, the exiſtence of this power in the ſyſtem is impoſſible, for it muſt have exiſted without a cauſe. Secondly, That the ſame phenomena are obſervable, where the ſuppoſed cauſe of them muſt be allowed to be abſent. And, thirdly, That if the ſuppoſed cauſe did exiſt, it would be followed by effects which are not found to take place.

Section 2d treats of the faculties of the ſenſorium. The author of Zoonomia conſiders the ſenſorial power as capable of exiſting in four different ſtates, to which he gives the names of Irritation, Senſation, Volition, and Aſſociation. But Mr Brown attempts to prove, 1ſt, That ſenſorial power, admitting its exiſtence, is incapable of change. 2dly, That admitting it to be capable of change, we are not juſtified in aſcribing ſimilar effects to diſſimilar cauſes; and that irritation, ſenſation, volition, and aſſociation, are not, therefore, different. 3dly, That admitting Dr Darwin's definition to be juſt, the parts of his diviſion are not oppoſed, but mutually involved. 4thly, That admitting the parts to be oppoſed, his diviſion is incomplete, as there are many phenomena of life, which cannot be reduced to any of the four claſſes of ſenſorial motions. And, 5thly, That Dr Darwin has not affixed preciſe ideas to the ſenſorial faculties, but uſes them in a different ſenſe, in different parts of his work.

In ſection 3d, The claſſes of fibrous motions are conſidered. The ſcope of our author's reaſoning in this ſection, is to point out two great errors of Dr Darwin, in his explanation of the manner in which fibres become equally excitable by irritations, ſenſations, volitions, and aſſociations. In the firſt place, he has overlooked, that although habit has the effect of rendering actions which have frequently occurred in ſucceſſion more eaſily performed; yet this influence does not extend to the whole train, for the ſame ſtimulus is neceſſary to excite thoſe fibres to contraction, the motion of which conſtitutes the primary link, after a thouſand repetitions, as when firſt excited, if the motion immediately preceding be different. There cannot, therefore, be any reverſe aſſociation. In the ſecond place, he alledges, that Dr Darwin had been led into error in ſtating the origin of ſenſitive and voluntary motions, by the ambiguity of his terms; ſenſations being ſometimes uſed by him generically, and ſometimes limited to a particular affection of ſenſorial power.

Stimulus and exertion are the ſubjects of the 4th ſection. The objections which Mr Brown urges againſt Dr Darwin's reaſoning on ſtimulus and exertion, are chiefly theſe: From Dr Darwin's data, he thinks it may be inferred, that in every affection of ſenſorial power, the ſenſorial power thus affected is expended. But if the whole be expended by the affection, fibrous contraction cannot take place. In irritation the ſenſorial power muſt be affected by the ſtimulus applied, before it excite the motion of the fibres; and if it periſh with the affection, the fibrous excitation will be impoſſible. If, on the contrary, the poſſibility of the propagation of ſenſorial changes in theſe circumſtances be admitted, and if ſenſation and volition be exertions or changes of the central parts of the ſenſorium, or of the whole of it, as Dr Darwin alledges, death muſt be the inevitable conſequence of either of theſe changes; for the ſenſorial power affected will be expended, and the ſecerning gland being thus void of the ſpirit of animation, will remain wholly inexcitable by the moſt violent ſtimulus. He aſſerts, that if nothing more be meant by the expenditure of ſenſorial power, than a ſlight change of its place in the ſyſtem, that may be admitted, without much ſtrength to the Doctor's theory. For if the general quantity of the ſpirit of animation,

mation be not diminiſhed by exertion, and if, at the ſame time, a continual ſupply of that fluid be ſecreted, the fibrous motions muſt continually increaſe in violence; and thus the phenomena, which ſeem to proceed from deficiency of ſtrength, are wholly inexplicable on the principles of the ſenſorial theory.

The ſuppoſition of the expenditure of ſenſorial power in organic motion, requires, to render the phenomena of animation explicable, another ſuppoſition which Dr Darwin has adopted, namely, That the quantity of the power expended in exertion is proportional to the violence of the ſtimulus. But, unleſs the ſenſorial changes be divided into thoſe which expend, and thoſe which do not expend ſenſorial power, it will follow from the principles of Zoonomia, that the quantity expended by the gentleſt ſtimulus is equal to that expended by the moſt violent. And even though it be admitted, that the ſenſorial power is expended by exertion, and that the quantity expended is proportionate to the violence of the ſtimulus, the phenomena of animal motion are not thereby rendered explicable.

Senſual motion forms the ſubject of the next ſection. According to Dr Darwin's theory, the immediate organs of ſenſe are compoſed of fibres, intermixed with ſenſorial power. They are ſtimulated to contraction, like the muſcular fibres from which they differ, in poſſeſſing a greater proportional quantity of the vital ſpirit. The motions of theſe fibres conſtitute our ideas; and when an organ is deſtroyed, the ideas of that organ neceſſarily periſh. But Dr Darwin, throughout his whole work, inſtead of limiting the ſenſe of the term Idea to that contraction, or motion, or configuration of the fibres, which conſtitute the immediate organ of ſenſe, uſes it occaſionally to expreſs alſo a ſtate of the ſpirit of animation, thus indifferently making the word Idea ſtand for the organic, and for the ſentient change.

On examining the facts from which Dr Darwin infers, that fibrous motions of the immediate organs of ſenſe conſtitute our notions of the qualities of external things, Mr Brown's aim is to ſhow, that the general error of the inſtances of analogy adduced is, that the author has confounded the cauſe with its effect, either conſidering the muſcular motion, inſtead of the motive affection, or the organic affection, inſtead of the idea, ſo that all his facts may be admitted, without furniſhing any additional ſtrength to his theory. Mr Brown next offers what he ſuppoſes poſitive proof, that the theory is not conſiſtent with the phenomena of mind; and, upon the whole, therefore, he alledges, that Dr Darwin's theory of ideas is wholly unſupported by evidence; that many phenomena exiſt which cannot be reconciled with it; and that many do not exiſt, the production of which would have been a neceſſary effect of the laws of ſenſorial motion. And he concludes, that as we do not render the affections of the ſentient principle more eaſy to be underſtood by reſolving them into motions of ſolids or fluids, the cauſe of motion being as inexplicable as the cauſe of the ſentient affection, Dr Darwin's principles, though they may be praiſed as ingenious, cannot be adopted as juſt.

Section 6th relates to the production of ideas. In this long and intereſting ſection, our author follows Dr Darwin's explanation of the different ſenſes, and with great ingenuity contends, that his explanations are either contrary to facts, or inconſiſtent with his own principles.

The claſſes of ideas are reviewed in the next ſection. Irritative ideas imply, Mr Brown imagines, a contradiction, becauſe perception and ſenſation are not eſſentially different. Senſitive ideas are ſtyled by Dr Darwin, thoſe which are preceded by the ſenſation of pleaſure or pain. But Mr Brown aſſerts, that no evidence can be adduced of the exiſtence of fibrous motions, excited in the organs of ſenſe by ſenſation. He thinks it a contradiction to ſuppoſe the exiſtence of voluntary ſenſual motions; for either the idea which it is wiſhed to recollect, exiſts previouſly to volition, or volition takes place without a determinate object. He concludes, that it is impoſſible to reduce the ideas originally excited by organic affection to any other claſſes than thoſe of perception and aſſociation.

Section 8th treats of vegetable animation. After having examined all Dr Darwin's arguments in defence of the animation of vegetables, Mr Brown infers, that there is not ſufficient evidence of their irritability, ſenſibility, voluntarity, and aſſociability; and hence that we are not juſtified in conſidering vegetables as animals differently organiſed.

In the 9th ſection, inſtinct is conſidered. By the term Inſtinct, Mr Brown imagines, ſhould be meant, in oppoſition to Darwin, a prediſpoſition to certain actions, when certain ſenſations exiſt. On this principle, he ſuppoſes all the inſtances adduced by Dr Darwin may be admitted, without the force of the argument in ſupport of inſtinct being thereby weakened. He then proceeds to notice ſome of the more intereſting facts enumerated by Dr Darwin. Theſe would lead us into too wide a range to review; we muſt not, however, omit what relates to the natural ſigns of our emotions, the examinations of the doctrines of Zoonomia on them being alſo included in this ſection. Theſe ſigns, Dr Darwin conſiders, not as natural, but as acquired; and he traces them chiefly to the aſſociations of early infancy. Fear, for example, which he aſſerts to be no other than the expectation of diſagreeable ſenſations, is excited from the experience of the uneaſy ſenſations of the infant at birth, occaſioned by the oppreſſion about his præcordia for want of reſpiration, and by his ſudden tranſition from 98° of heat into ſo cold a climate. But to this theory our author objects, that fear implies a degree of knowledge, which the infant, before its entrance into the world, cannot have acquired. It is conſcious of diſagreeable ſenſations, but of nothing more; for fear, which is the expectation of diſagreeable ſenſation, cannot exiſt till after the ſenſation have been experienced, and the animal changes have ceaſed.

The natural ſigns of grief, which are ſniveling at the noſe, a profuſion of tears, and ſome peculiar diſtortions of countenance, are explained in Zoonomia upon the principle, that the aperture of the lachrymal ſac of the newly born infant, being diſagreeably affected by the coldneſs and dryneſs of the atmoſphere, the tears contained in it are poured into the noſtrils, while others are ſecreted and diffuſed over the ball of the eye. The diſtortions are occaſioned by our lowering the brows, and compreſſing the gland, when we wiſh a quick ſecretion. On this explanation, Mr Brown remarks, that as the diffuſion of tears on the noſtrils, and the eye-ball, is aſcribed to a cauſe which acts at the moment of birth, the natural ſigns of grief and fear having co-exiſted at the time of the original aſſociation, ſhould, according to the theory, form one train; and hence fear ſhould be expreſſed by ſniveling and tears. And as the infant can have no deſire of increaſing the quantity of his tears, nor, though he have the deſire, can he know the means of aiding the ſecretion, the diſtortions of countenance cannot be referred to early aſſociation.

Tender pleaſure is, by Dr Darwin, ſuppoſed to be attended by tears, becauſe the tears excited by the fragrance of its mother's boſom, are ſucceeded in the mind of the infant by affection. But in the work before us, it is contended, that if any aſſociation be formed at that time, ſo ſtrongly as to continue during life, it is more probable, that tears ſhould invariably produce affection. The ſmile of ſerene pleaſure, Dr Darwin aſcribes to the relaxation of the ſphincter of the mouth, when it has been fatigued by the action of ſucking. Mr Brown, however, alledges, that if the natural ſigns of theſe ſpecies of pleaſure are to be traced to the aſſociations of infancy, the ſigns might with greater reaſon be reverſed, the tender feeling being expreſſed by a ſmile, and the ſerene by a tear. For the tears excited by the odour of the milk, are ſooner ſucceeded by the ſerene pleaſure or ſucking, than by affection, and ſhould therefore more readily become the ſign of the former; while the ſmile is ſooner ſucceeded by affection, than the irritation of the noſtrils, and ſhould therefore more readily become its ſign.

In the ſame manner, Mr Brown controverts Dr Darwin's explanations of the ſigns of anger, attention, violent pain, and tumultuous pleaſure. He concludes, that it may be objected to the whole of the theory of natural ſigns, contained in Zoonomia, that it is founded on artificial language, and not on nature. In nature there are no claſſes. The names of fear, and tender pleaſure, are given to various feelings, but there exiſt only particular feelings of fear and pleaſure. Though certain motions be aſſociated with one feeling, they cannot, on the principle of aſſociation, become the ſigns of other feelings, unleſs a particular aſſociation of each have taken place; which is inconſiſtent with Dr Darwin's theory. It may therefore be admitted, that the origin of the expreſſions of the fear of cold, is rightly explained; but the early co-exiſtence of motions, on which this is founded, cannot explain the application of the ſame ſigns to the different ſtate of mind, which conſtitutes the fear of fire, and the fear of murder.

The catenation of motions is the ſubject of the 10th ſection. Dr Darwin, in his ſection on animal cauſation, ſtates three modes of connection between fibrous and ſenſorial motions, namely, 1. The immediate ſucceſſion or co-exiſtence of fibrous contractions, termed Aſſociation. 2. The ſucceſſion of fibrous to ſenſorial motions, termed Cauſation.

tion. 3. The reciprocal production of fibrous and ſenſorial motions, termed Catenation of animal motions. In Mr Brown's opinion, however, all the aſſociate actions of which Dr Darwin had treated previous to the preſent action, are catenations of animal motions; and hence, although the terms he applies be different, the phenomena are the ſame. Our author then controverts the ſeveral propoſitions in Zoonomia, relating to the catenation of motions, premiſing the general principle, That no laws can be inferred of a ſucceſſion of motions, which are not applicable to the parts of the brain.

The next four ſections relate to ſleep, reverie, vertigo, and drunkenneſs. With regard to ſleep, Mr Brown firſt objects to the aſſertion, that the power of volition is totally ſuſpended. Thus, Dr Darwin obſerves, that thoſe who are ſubject to epileptic fits, are moſt liable to them during ſleep; but this implies, that voluntary motions are moſt frequently excited, when voluntarity is ſuſpended. Again, if deſire and averſion conſtitute volition, theſe are allowed to exiſt during ſleep, and volition cannot therefore be ſaid to be ſuſpended. Further, the circumſtances which induce or prevent ſleep, are alledged by our author, to be in direct oppoſition to Dr Darwin's theory. And from a conſideration of theſe circumſtances, he infers, that if ſenſorial power be the ſource of life, they rather ſupport the contrary opinion, that an increaſe of that fluid, and conſequently of voluntarity, is the proximate cauſe of ſleep.

That the ſenſitive motions proceed with greater livelineſs, is an aſſertion, Mr Brown thinks, equally unfounded as the former. And he controverts alſo the propoſitions, that irritative ſenſual motions are not excitable, and that the firſt link of our aſſociations is not excited by external ſtimuli during ſleep. In oppoſition to Dr Darwin, he ſuppoſes, that the phænomena of ſleep differ in no very conſiderable degree from thoſe of the waking mind. The change appears to be rather bodily than mental.

In his explanation of Reverie, it is aſſerted, that Dr Darwin has contented himſelf with ſtating phenomena, without attempting to reconcile them with his theory of life.

The term Vertigo is employed by Dr Darwin in a more comprehenſive ſenſe than that in which it is uſually underſtood, being applied to the irregular, aſſociate motions of various organs. The viſual vertigo, however, is more particularly conſidered by the Doctor as its phenomena, are more frequent and ſtriking, and ſeem to aid us by their analogies, in developing the cauſes of the other ſpecies. Our author, therefore, is chiefly anxious to examine his explanation of that ſpecies of vertigo. He alledges, that the inſtability of our own body is occaſioned by a deception of the imagination, as we believe ourſelves to be ſtill revolving after the original motion has ceaſed; we, therefore, fix our foot at a greater angle, to counteract the ſuppoſed motion; and this proving ineffectual, we voluntarily fall on the ground, as the ſureſt mode of preventing it. At any rate, Dr Darwin's theory, that it is occaſioned by the incapacity of balancing ourſelves, from the abſence of viſual ideas, cannot be juſt; for we walk in the dark when no viſual idea exiſts, without ſtaggering, or the ſlighteſt inclination to fall to the ground. The apparent retrogradation of external objects, inſtead of being occaſioned by ſpectra, Mr Brown imputes to the ſame deception of imagination; for if we believe ourſelves to be ſtill in motion, after we have ceaſed to revolve, it is a neceſſary conſequence, that the objects which affect our organ of viſion, appear to move in the oppoſite direction. In the explanation of the phenomena of vertigo in its moſt extenſive ſenſe, he accuſes Dr Darwin of having been deceived by conſidering the general fact, rather than its particulars. Irritative motions of the different organs, he remarks, may perpetually exiſt; but theſe motions, as their degrees are different, are not connected with each other. They have not been formed into tribes, and cannot therefore conſtitute a great circle of motion.

The theory, that drunkenneſs conſiſts in diminiſhed voluntarity, is overturned, it is urged, by a fact adduced by Dr Darwin himſelf in its ſupport. Some inebriates are ſaid by him to be inſtantly rendered ſober by any accident which ſtrongly excites their attention, as the pain of a broken bone, or the news of their houſe being on fire. In other words, our author adds, drunkenneſs conſiſts in the diminution of ſenſorial power, and the inebriate is rendered ſober by that which diminiſhes ſtill more the ſenſorial power in his ſyſtem. Other arguments are offered to ſhow, that the theory is unſupported by facts, and inconſiſtent with the other principles of Zoonomia.

Section 15th relates to propenſity to motion, repetition, and imitation. The accumulation of ſenſorial power, in whatever mode it be induced, is ſaid by Dr Darwin to be attended with propenſity to motion. But it is contended, that the accumulation may increaſe particular motions, but cannot produce a propenſity to motion.

The greater facility of motion, after frequent repetition, is aſcribed, in Zoonomia, to the union of aſſociation with the original motive faculty. But aſſociation cannot unite with any other faculty; for if it exiſt, it muſt itſelf induce the particular motion; and the other faculty, inſtead of aiding repetition, will thus prevent it.

The propenſity to imitation, which gives the chief impulſe to the infant mind, and which ceaſes not to operate, though in a leſs degree, when the years of infancy have elapſed, is aſcribed by Dr Darwin to the greater eaſe of repeating with another organ a ſet of actions, imitated by the retinal fibres, than of inventing an action which bears no reſemblance to preceding motions. This imitation, however, muſt be a voluntary action, and if the action be wholly new, the law of Zoonomia is not juſtly inferred, which aſſerts all our motions to have been originally irritative. Mr Brown next offers ſeveral arguments in ſupport of the opinion, that curioſity is the great cauſe of the actions which ſhould properly be ſaid to reſult from imitation; and that aſſociation of ideas is the cauſe of many actions which have been ſuppoſed to be that of imitation.

The retrograde motion of the abſorbent ſyſtem, forms the ſubject of the 16th ſection. If ſuch a motion do take place, it is urged by our author, that it is wholly inconſiſtent with the general principles of the theory.

In ſection 17th, Dr Darwin's explanation of the temperaments is examined. It is remarked, that the Doctor ſeems to have forgotten, on this occaſion, that irritation, ſenſation, volition, and aſſociation, are only modes of exertion of the ſame ſubſtance; and that propenſity to action, whether it be called irritability, ſenſibility, voluntarity, or aſſociability, is only another mode of expreſſion for the quantity of ſenſorial power reſiding in the organ to be excited. It is therefore impoſſible to diſtinguiſh temperaments by the increaſe or diminution of theſe energies, as oppoſed to each other, ſince the cauſe which diminiſhes one muſt diminiſh all.

The theory of nutrition, which is conſidered in the next ſection, tends, in our author's opinion, to eſtabliſh the immortality of animals and vegetables, if the ſenſorial gland be not deſtroyed by external violence, nor the ſupply of oxygen and food impeded.

The diſquiſitions on generation, contained in ſection 19th, are too extenſive to admit of analyſis in this article.

In the next ſection Mr Brown endeavours to ſhew, that madneſs does not conſiſt, as Dr Darwin aſſerts, in the increaſed volition of our organs of ſenſe; and that there is even no proof, that the conſequent exertions are greater than ought naturally to be produced by the exiſting ideas. Madneſs, he obſerves, is a diſeaſe of the motives alone. In other words, then, we are not excited by the real appearance of things, but by the more vivid ideas of imagination.

Section 21ſt is allotted to the conſideration of fever. In this ſection, Mr Brown attempts to ſhew, that Dr Darwin's theory of fever is founded on a miſtaken view of the qualities of the ſpirit of animation, as ſtated in the introductory ſections of Zoonomia.

In the laſt ſection, a number of miſcellaneous obſervations are offered, upon different paſſages of both Dr Darwin's volumes.

We have thus endeavoured to exhibit a ſummary view of this ingenious volume. We regret, however, that our limits prevent us from making ſuch an analyſis as the intrinſic value of the work deſerves, or of offering

ing ſuch a critique as is due to the merit of the author. We have remarked with great pleaſure, that although Mr Brown has combated, in energetic terms, the leading principles of Zoonomia, he has uniformly employed temperate language, and has invariably treated Dr Darwin with reſpect.

XX.

De l'Action Spontanée de l'Acide Sulfurique concentré ſur les Subſtances Vegetales et Animales. Par les Cit. Fourcroy et Vauquelin.

See Annales de Chimie, No. 68. *Auguſt* 1797. *Tome* XXIII. 8*vo.* Paris.

IT is with the utmoſt pleaſure that we notice the preſent Eſſay, particularly as it is announced as part of a work on vegetable analyſis, which the ingenious and indefatigable authors are preparing.

When any dry vegetable ſubſtance, as wood, ſtraw, or gum, is plunged into concentrated ſulphuric acid, it becomes coloured, ſoftens, and ſeems to diſſolve in the acid, forming a kind of magma, or pulp, of a black or brown colour. If, after their reciprocal action is at an end, the mixture be diluted with water, two very intereſting appearances are obſerved, the precipitation of an almoſt perfectly pure carbon, and the much ſmaller evolution of heat than what would have reſulted from the mixture of the water with the ſame acid, previous to its action on the vegetable matter.

The vegetable matter itſelf is certainly much altered, for its carbon is ſeparated almoſt pure. But our authors found, that the ſulphuric acid itſelf remained undecompoſed. What then broke the reciprocal attraction of the carbon, hydrogen, and oxygen, of the vegetable matter employed? On ſeparating the carbon, it will be found, that the ſulphuric acid, ſwimming over it, is greatly weakened, and that it contains acetous acid, which may be procured by diſtillation. The vegetable matter is therefore converted into acetous acid and carbon. But by comparing the quantities of theſe new products, with that of the original matter, a conſiderable loſs will be detected; and as there was nothing loſt in the experiment;

no extrication of any elaſtic fluid, it is evident, that the water, to which the diluted ſtate of the ſulphuric acid is owing, muſt have been formed at the expence of the conſtituents of the vegetable.

We muſt therefore conclude, that vegetable matter, treated with cold concentrated ſulphuric acid, is decompoſed; that one part of its carbon is ſeparated, almoſt pure; that another part, with hydrogen and oxygen, forms acetous acid, while the reſt of the hydrogen and oxygen compoſe the water with which the ſulphuric acid is diluted. All theſe changes are at the expence of the conſtituents of the vegetable itſelf. It is only a variation of equilibrium amongſt them, a diviſion and unequal combination of the component parts, which has generated the three new products. The cauſe determining this new arrangement, is evidently the ſtrong attraction of the concentrated ſulphuric acid for water, and is another remarkable example of what has lately been denominated Diſpoſing Affinity.

The reſult of this experiment is conciſely expreſſed in algebraic language, in the following formula: Diluted ſulphuric acid+acetous acid+precipitated carbon=concentrated ſulphuric acid+vegetable matter entire. It is not, however, always ſo ſimple: ſometimes two vegetable acids are formed, ſometimes a little alcohol, and in ſome caſes carbonated hydrogen gas is extricated. But theſe are only occaſional phenomena, and depend on the different proportions of the principles compoſing the vegetable, eſpecially on a large quantity of hydrogen. Where there is a remarkable deficiency of oxygen, the ſulphuric acid itſelf is attacked, and muſt part with a ſufficient quantity of it to unite with the hydrogen into water, and ſulphureous acid is formed. This is particularly the caſe with oily bodies.

On account of the more complicated nature of animal matter, the reſult of their immerſion in concentrated ſulphuric acid, is alſo more complicated. No volatile principle flies off. The azot unites with a portion of the hydrogen, and forms ammonia, which combines with the ſulphuric acid, or that new one whoſe formation takes place at the ſame inſtant. Another part of the hydrogen forms, with the oxygen, the water to ſaturate the ſulphuric acid, and the ſuperabundant quantity of carbon is precipitated.

Thus, in caſes of burns by ſtrong ſulphuric acid, which are often ſo ſevere and dangerous, there are formed, at the expence of the texture of the ſkin, water, vegetable acid, and ammonia, with a precipitation of carbon; and theſe ſimultaneous effects produce a complete deſtruction of the parts burnt, as is evident from the brown or black ſcar, which ſoon ſeparates, and falls off as a foreign ſubſtance.

It likewiſe ſhews how great the diſorganizing energy of this concentrated acid muſt be upon the ſtomach and œſophagus, and the utility of preſenting to it, the moment it is ſwallowed by any accident, a mucous liquid, on which it may directly and quickly exhauſt its activity.

XXI.

De l'Action de l'Acide Sulfurique ſur l'Alcool, et la Formation de l'Ether. Par les Cit. Fourcroy et Vauquelin.

See Annales de Chimie, No. 68. *Aout* 1797. 8vo. Paris.

THIS paper may be conſidered as a continuation, and practical application of the former. After tracing, with great accuracy and minuteneſs, the phænomena which occur during the formation of ether, our authors deduce the following concluſions.

1ſt, The formation of ether is not owing to the immediate action of the principles of the ſulphuric acid, on thoſe of the alcohol, according to the general opinion, but to a real re-action of the elements of the alcohol upon each other, particularly of the oxygen and hydrogen, ſimply occaſioned by the ſulphuric acid.

2dly, Any quantity of alcohol might be changed into ether without heat, by ſufficiently augmenting the quantity of ſulphuric acid.

3dly, The operation is divided into two parts; during the firſt, only ether and water are formed; during the latter, ſweet oil of wine, water, and acetous acid.

4thly, While ether continues to be formed, the ſulphuric acid is not decompoſed, and no oil of wine produced; but as ſoon as this latter appears, the ſulphuric acid is decompoſed, and no more ether, or at leaſt very little, is produced.

5thly, The formation of oil of wine may be prevented, by keeping the temperature at 75 to 78° degrees, by dropping, occaſionally, ſome drops of cold water into the retort.

6thly, Alcohol differs from ether, in containing more carbon, and leſs hydrogen and oxygen; and oil of wine is to ether what alcohol is to oil of wine.

XXII.

Memoire ſur l'Opium. *Par* Joſſe.

See Recueil Periodique de la Société de Santé de Paris, No. II. *Brumaire, an.* V. *or Nov.* 1796. 8vo, Paris.

THE following proceſs is deſcribed by the author for obtaining the extractive part of opium pure, and free from all heterogeneous matters.

After having choſen the maſſes, and cleaned them of the leaves, they muſt be divided into pieces weighing about an ounce. Theſe are to be ſucceſſively plunged into water, at the heat of 30 to 35 Reaum. held in the hand and manipulated, according to Beccaria's method for obtaining the gluten from paſte. The quantity of water, in order to ſave time and expence in the future evaporation, ought to be about four times the weight of the opium. The ſurface of the opium ſoftens, and is covered with a fibrous matter, which adheres a little to the ſkin. By continuing the preſſure, the water becomes tinged of a dirty brown; the fibrous matter increaſes in quantity, and changes under the hand into an elaſtic ſubſtance, which adheres to the ſkin when the water is too warm.

After all the opium has been uſed, and the fibrous matter collected, it ſhould be again well preſſed in cold water, to free it entirely from the extractive part ſoluble in that menſtruum. This our author names the virulent glutinous matter of opium. It is of a brown colour, and its taſte and ſmell are ſo pungent as to excite vomiting. Kept in a dry place, it loſes its elaſticity, and puts on the appearance of a brown earth; when confined, ſo that its moiſture cannot evaporate, it undergoes a kind of putrid fermentation, and acquires a cadaverous ſmell.

The ſolution is to be ſtrained through paper, to ſeparate ſome portions of glutinous matter ſwimming in it, and to be evaporated with a heat under the boiling point. When about the half is conſumed, it muſt be again filtred and left to cool. On cooling it grows turbid, and on ſtanding depoſits a reddiſh brown matter, of the conſiſtence and tenacity of pitch, adhering ſtrongly to the ſides of the veſſel. The clear liquor is now to be evaporated in earthen veſſels with a low degree of heat.

The reſult of this operation is a dark brown extract, almoſt without ſmell, and very little like that of opium; of a bitter and ſomewhat ſalt taſte, without being nauſeous; it is deliqueſcent, ſo that it muſt be kept in well-cloſed veſſels; and in quantity about half the original weight of the opium. It is ſoluble in water, wine, and brandy, but ſcarcely in rectified ſpirit, from whence it is alſo depoſited after ſome time. It acts with certainty in the doſe of a grain, and is, our author ſays, the moſt gentle calmer, having nothing of that ſtupifying convulſive action, which opium not freed from the glutinous part has. He alſo gives ſome formulæ for the uſe of this extract.

The matter depoſited after the firſt evaportion, is, according to our author, a true reſin, perfectly ſoluble in rectified ſpirits, or in ether, but ſcarcely in brandy. About a drachm is obtained from eight ounces of opium, which diſſolves completely in one ounce of rectified ſpirit, and by evaporation affords a dry, brittle reſin, of a dark brown colour, with the nauſeous taſte and ſmell of opium. This reſin, our author thinks, may be of great ſervice, from its being eaſily miſcible with unctuous ſubſtances.

A pound of opium affords about ſix ounces and an half of the virulent glutinous matter, which is reduced, by perfect deſiccation, to five ounces and a few drachms. Put into a retort, a ſmall degree of heat brings over a few drops of colourleſs phlegm, having a ſuffocating ſmell of opium, and affecting the head; afterwards a phlegm more coloured, with a few drops of amber-coloured oil. In the ſequel of the operation, concrete ammonia is ſublimed with ſome empyreumatic oil, and there remains in the retort a ſpongy charcoal, like that of animal fleſh. By the pneumatic apparatus, different gaſes may be collected, firſt carbonic acid, then ammoniacal gas, and, laſtly, an inflammable gas, which is, however, only volatilized empyreumatic oil.

The glutinous matter, when immerſed for ſome time in water, ſwells, and emits bubbles of an elaſtic fluid. The water becomes ichorous, and takes on the alkaleſcent fermentation, as animal matters, and gives out, with the ſmell of opium, a moſt diſguſting fœtor.

Digeſted with diluted ſulphuric acid, ſulphat of ammonia is obtained.

One ounce of it, divided into ſmall portions, communicated to four ounces of ſpirit of wine a reddiſh brown colour, and the nauſeous taſte of opium. After ſeveral days, the portions ſtill retained their ſize and form; but on being dried, were found to have loſt about an eighth of their weight. On evaporation of the tincture, half a drachm of a very bitter reſin, with little ſmell, was obtained.

When dried, it may be fried in boiling oil, without colouring it, but if moiſt the oil becomes green. The mineral acids convert it into charcoal, and the gaſes given out have always the fœtid ſmell of opium.

It diſſolves, like glue in vinegar, in fermented liquors, containing much tartar, and in acid vegetable juices. Theſe ſolutions are decompoſed by carbonat of potaſh, and the fibrous matter is reproduced, ſtill retaining the diſtinguiſhing ſmell of opium. By this analyſis it is evident, that the glutinous matter reſembles very much that of wheat.

From the whole, our author deduces the following pharmaceutic corollaries.

1ſt, That the preparation of opium with acid fruits, only ſerves to diſſolve the glutinous matter more completely, and hence the ſtupor ariſing from their action.

2dly, That the laudanum of Rouſſeau, made by the fermentation of opium with honey, is without the glutinous matter, as is proved by the nature of the fœces.

3dly, That the only vinous menſtruum fit for opium, is Spaniſh wine, from its containing no tartar, and from the abundance of its ſaccharine mucus.

And, laſtly, That from the extenſive culture in ſeveral of our departments of the large poppy, to get the oil of the ſeeds, called *Huile d'Oeillet*, apothecaries could form a ſubſtitute for foreign opium, from the juice of that plant, gathered in the time of its greateſt vigour, defœcated according to the rules of art, and formed into an extract, which, our author affirms, may be given in the ſame doſe, and with the ſame ſucceſs, as the opium of commerce.

From a few experiments we have made, agreeably to this analyſis, we are inclined to doubt the accuracy of the author's obſervations. The glutinous part of the opium, to which he aſcribes ſuch virulent qualities, appears to us to be a nearly pure *gluten vegeto-animale*, whoſe colour, taſte, and ſmell, depend entirely on ſome portion of the reſinous and extractive matter ſtill adhering to it. The pure reſin, obtained by precipitation with water from the tincture, is taſteleſs, while, on the contrary, the extractive matter is intenſely bitter, has the ſmell characteriſing opium, and ſeems the only active conſtituent of the drug.

XXIII.

Eſſai ſur les Proprietes Medicinales de l'Oxigene, et ſur l'Aplication de ce Principe dans les Maladies Venerennes Pſoriques et Dartreuſes ; preſentó a la Societé de Medecine de Paris, le 7 Meſſidor, de lan V. de la Rep. Franc. Par le Citoyen Alyon, *Membre de la Societé Medicale d'Emulation de Paris, officier de Santé de l'Hoſpital du Val-de-grace, et ancien eleve de Fourcroy.* 8vo. Paris.

THE medical theories of all ages have moſt commonly been borrowed from the prevailing philoſophical opinions of the time. It was therefore natural, that the beautiful diſcoveries of Lavoiſier, and the revolution he effected in the ſcience of Chemiſtry, ſhould influence the reaſonings of phyſicians; and accordingly, in our own country, ſeveral ingenious men have adapted ed his principles, with ſucceſs, to the explanation of the functions of life, and the origin of diſeaſe. But in France many partial cauſes, ſuch as national eſteem, and univerſal enthuſiaſm, concur to render its effects ſtill greater, of which we have an example in the Eſſay now before us.

Citizen Alyon employs his preface in informing us, that his doctrine ſubverts all thoſe which have preceded it; that he diſregards prejudices, ſanctioned by three centuries; that he endeavours to eradicate ancient errors, recorded in the moſt celebrated works; in a word, that he aims at baniſhing mercury from the treatment of ſyphilitic complaints. The little progreſs we have made in this branch of the healing art ariſes, in our author's opinion, not leſs from the fatal veneration we have for mercury, than our ignorance of its mode of action.

Fourcroy's lectures, and ſtill more his converſation, led our Author to a train of experiments, which left no doubt, he ſays, of the manner in which mercury acts, and to the diſcovery of ſome combinations of oxygen fitted to ſupply its place, without being attended by its inconveniences.

Fourcroy has long hinted in his lectures, that the mercurial preparations owed their activity ſolely to the oxygen they contained; but undeniable facts and deciſive experiments were wanting to ſupport his opinion. Theſe, our author flatters himſelf, he now brings forward; but, to put the ſuperiority of the new method of treating venereal complaints beyond a doubt, he has demanded from the Miniſter of the Interior, the inſtitution of a comparative trial, and has been appointed one of the commiſſaries for carrying it into effect. As ſoon as it ſhall be completed, he promiſes to make its reſult known.

His preſent Eſſay is introduced by ſome obſervations on oxygen, and its uſes in the animal and vegetable economy. Its employment in medicine was ſuggeſted, he tells us, by reflecting on the phenomena of oxidation, the anti-ſyphilitic power of the red oxyd of mercury, and the affinities of oxygen, to which principle he was ſoon convinced all mercurial preparations owed their efficacy. Conformable with his ideas, in order to obtain a powerful anti-ſyphilitic remedy, an active ſtimulus, capable of changing the ſtate of the ſyſtem, it is only neceſſary to procure a ſubſtance which contains much oxygen, and yields it readily to animal ſubſtances.

With this view, our author endeavoured to unite as much oxygen as poſſible with fat, without decompoſing it, and at laſt ſucceeded in preparing what he terms Oxygenated Ointment. Sixteen parts of freſh lard are melted in a glazed earthen veſſel, with a moderate degree of heat, and when melted, two parts of pure nitric acid of 32° are poured upon it; the heat is maintained till the mixture boils, when the veſſel is removed from the fire, and permitted to cool.

In this operation, the nitric acid is completely decompoſed. Two ounces of fat, treated with two drachms of acid, acquired one drachm in weight, and pure azotic gas was collected in a proper apparatus. Although the proceſs ſeems extremely ſimple, it requires, he tells us, conſiderable dexterity to form an ointment always ſimilar in its effects and appearance. The greateſt difficulty is to catch the moment when the decompoſition of the acid is completed; for the ſmalleſt increaſe of heat at that time, diſunites its principles, and ſeparates a quantity of carbon.

When well prepared, this ointment has no taſte, and yields to the action of water neither nitric nor ſebacic acid, is of a yellowiſh white colour, of a conſiſtence between ſuet and virgin wax, melts without alteration, and facilitates the oxydation of metals rubbed with it. If the acid be in exceſs, the ointment will not become firm, and its uſe will inflame the ſkin; if it be in too ſmall quantity, it will be leſs active.

But fat may be made to decompoſe a greater quantity of acid, by adding it in different portions. The ointment then has little ſmell, is of a higher colour and firmer conſiſtence, ſpreads eaſily on linen. This is preferable in diſeaſes of the ſkin, in herpetic and venereal ulcers, and may be uſefully employed in healing wounds and atonic ulcers, while the leſs oxygenated ointment is better adapted for diſeaſes of the face.

Theſe ointments, ſays our author, readily part with ſome of their oxygen to the parts on which they are rubbed, they ſtimulate the muſcular fibre, and increaſe its tone, they inſpiſſate the ſerous fluid which runs from the ſores, and thus induce cicatrization. Their antipſoric properties are, he ſays, univerſally acknowledged, but are moſt remarkable in moiſt, ſcabby itch.—Herpetic, eryſipelatous, and incipient glandular diſeaſes, yield alſo to their application.

Citizen Alyon next proceeds to treat of the nitric acid, and its medical properties; obſerving the facility with which it was decompoſed by fat, he was firſt led to imagine, that it would alſo part with its oxygen eaſily, and gradually, when taken into the ſyſtem; and the ſucceſs of his trials have, he ſays, ſurpaſſed his expectations.

Its effects, when diluted, are to increaſe gradually and inſenſibly the vital power, to augment the appetite, to promote the flow of urine, and to change the ſtate of the ſyſtem. He has adminiſtered it, he tells us, to one hundred and fifty patients in the ſpace of eight months; and if it has not been always ſucceſsful, it has never occaſioned the ſmalleſt inconvenience. As it increaſes the inſenſible perſpiration, he recommends warm cloathing, and attention to keeping the feet dry during its uſe.

In primitive venereal ſymptoms, he thinks the daily uſe of half a drachm, diluted with a pint of water, ſufficient. Its effects are not always equally rapid, but in general he thinks that it acts more certainly and quickly in old venereal complaints, whatever their ſymptoms may be. Its uſe produces in ſome people a kind of ſalivation, more or leſs abundant, which, however, is never exceſſive, nor bears any reſemblance to that cauſed by mercury. Another conſtant effect of the acid, according to our author, is to develope the action of the morbific matter, to make ſymptoms appear which were not before viſible, and to diſcover any virus which may have been abſorbed; ſo that he recommends its uſe particularly to thoſe people who doubt of their being perfectly cured.

Our author next details twenty-ſix caſes in which he uſed theſe oxygenating remedies. Moſt of them are obſtinate inſtances of herpes, or venereal complaints, many of which had already reſiſted the uſe of mercury. He generally gave a drachm of nitric acid, diluted with a pint of water, daily, and applied his oxygenated ointment topically, or touched the ſores with a ſolution of the oxymuriat of potaſh or of antimony.

A young man, aged twenty-one, conſulted our author for a phymoſis, which was ſo conſiderable, that he feared the mortification of the part. He was ordered the warm bath, and to immerſe his penis in tepid water. Next day he was able to uncover the glans, though with difficulty, on which there were four chancres, two of them very deep, and there were twelve others on the internal ſurface of the prepuce. Theſe were all touched with a very ſtrong ſolution of oxymuriat of potaſh. Next day, their appearance was entirely changed, and they had loſt their white edges. They were touched in the ſame manner five ſucceſſive days, when the ſwelling had entirely ceaſed, and ſeveral of them had diſappeared. They were now dreſſed with oxygenated ointment, ſpread on fine linen; and on the fifteenth day he was perfectly

cured.

cured. During the whole treatment, he had taken a pint of water acidulated with the nitric acid, daily, which he continued fifteen days after the chancres were cured. Seven months afterwards, the patient was perfectly well, and had had no relapſe.

A woman of thirty years of age, ſix weeks after being delivered of a ſyphilitic child, which lived but a few days, conſulted our author. The labia and nymphæ were covered with a number of chancres, and there was a copious green running. She had a ſallow complexion, could ſcarcely digeſt any thing, had continual headachs, ſlept none, and could hardly walk. She was ordered to drink a pint of the nitric ptiſane daily, and to waſh herſelf frequently with tepid water. On the eighteenth day, her headach diſappeared, her ſtomach had reſumed its functions, ſhe ſlept well, complained no longer of debility, and had got her natural colour. Only two chancres remained. She continued her drink to the thirtieth day, at which time all the ſymptoms diſappeared, and ſhe has had no relapſe, now, at the end of five months.

Cit. N. ſervant of Gen. Hedouville, had four chancres and a gonorrhœa. The chancres were touched with a ſolution of oxymuriat of potaſh, and dreſſed with oxygenated ointment, and he took a pint of nitric julep daily. In ten days two of the chancres had diſappeared, one of the others, which was very deep, and of a bad character, was touched with oxymuriat of antimony; next day it had a healthy appearance, and they were both ſoon healed. He continued his julep till the thirty-ſixth day, when his running ſtopped.

F. a ſoldier, aged twenty-eight, had a conſiderable bubo, which occupied the ſuperior glands of the groin; his pulſe was febrile, his ſkin of a leaden hue, and he could ſcarcely walk. The bubo was covered with oxygenated ointment, but on the eighth day became diſcoloured. An emollient cataplaſm was applied, and in five days it burſt. The cataplaſm was continued for ſome time, and the oxygenated ointment again applied, which cicatrized the ſore in eight days. The acid was, however, continued till the thirtieth, when his appetite and ſtrength were wonderfully recruited. It is now four months ſince, and he has had no return of his complaints.

To the caſes which fell under our author's own obſervation, others are ſubjoined, communicated by Lemoine, Boutin, and Marc. He next gives an account of the experiments made in Britain with the nitric acid; and concludes with inquiring into the modes of action of the venereal poiſon, the preparations of mercury and other oxygenating remedies, on the ſyſtem.

SECT. II.

MEDICAL OBSERVATIONS.

I.

Hiſtories of Singular Caſes of ruptured Uterus and Vagina. By the late Dr Roſs, *Phyſician at Hamburg. Communicated to* Dr Duncan *by* Dr James Carmichael, *Port-Glaſgow.*

CASE I.

SUPPOSED LACERATION OF THE VAGINA DURING PERTURITION.

HAMBURG, 1ſt July, 1785,—I was called to the wife of Mr Granan, clergyman in Eppendorf, about two Engliſh miles from this place.

Mrs Granan had been in labour of her fourth child ſince 11 o'clock in the morning, with moderate pains. But the midwife finding, that, beſides the head, a hand preſented, Mr Granan thought proper to ſend for other aſſiſtance. Notwithſtanding this, the midwife, imagining that ſhe could deliver the woman before any one came, made an attempt to reduce the hand. During which time, the patient ſaid ſhe had had four very ſtrong pains, and felt as if ſomewhat was torn, and as if a conſiderable change had taken place within her; the child mounting higher, and at firſt moving violently; but that the motion of the child ſoon afterwards ceaſed entirely, and the labour-pains alſo.

The midwife obſerved a change in the appearance of the belly. The ſwelling, which had occupied the lower part, now extended acroſs the upper, and ſeemed divided, giving her the idea of twins.

Profeſſor Unzer, a phyſician of Altona, who arrived at the houſe before me, and had already examined, informed me, that he had felt the inteſtines; and that the uterus was, in a great meaſure, torn off from the vagina; that in this ſituation of affairs, he had made an unfavourable prognoſtic; and waited for my coming.

This change, or laceration, (if what is above or afterwards related can convince the reader that it was ſuch), took place about four o'clock. It was now three quarters paſt ſeven. No hæmorrhage; no faintings, or other bad ſymptoms; the pulſe natural.

After a ſhort conſultation, ſuppoſing it to be the only mean for ſaving mother and child, provided the latter was yet alive, we determined for the delivery.

The lady being properly placed, it was agreed upon that I ſhould examine; and, at the ſame time, if I found the rent large enough for the purpoſe, and that it was practicable, then I was to deliver by turning. Otherwiſe, we were to propoſe gaſtrotomy.

Before I ſat down to examine in the vagina, I caſt my eye on the abdomen, and felt it, in order to determine the poſition of the child, and which hand to introduce.

The child was more extended from one hypochonder to the other; and the parts likewiſe appeared, and were felt more diſtinctly through the integuments than uſual. I was pretty certain, that the upper parts lay to the left, the feet to the right of the mother; therefore that the right hand was to be introduced.

In paſſing up the hand, I met with a looſe pendulous body in the pelvis; which, as inteſtines had been mentioned, I thought might poſſibly be ſuch, or the edge of the placenta. However, without particular examination, I paſſed my hand up behind it, and beyond the brim of the pelvis, without feeling the os uteri, or meeting with any other reſiſtance. The points of my fingers now touched the child immediately, without any intervening ſubſtance.

On examining the part, I found it was an arm. I paſſed along the body of the child, towards the woman's right ſide; and, getting to the extremity of a limb, I reached the right foot, which I brought gently down. The body eaſily followed, without bringing down the other foot; the back of the child to the fore-parts of the mother.

In attempting to bring down the left arm, I met with ſome reſiſtance; therefore I extracted the right, and afterwards (but ſtill with ſome difficulty) the left. The head eaſily followed. There was no pulſation in the funis; the child as white as uſual after uterine hæmorrhage, from a previous ſeparation of the placenta. I have ſeen children born alive, even by turning, where the mother was irrecoverably exhauſted by a many hours continuance of uterine hæmorrhage, from a partial ſolution of the placenta; and others loſt in a ſhort time, apparently, from that cauſe, where it had not been neceſſary to accelerate the birth, where the child had ſuffered no violence, and the mother was not very much reduced.

I introduced my hand again; met with the ſecundines pretty high above the pelvis, quite detached all around. But I felt nothing of the fundus uteri, nor any thing circumſcribing the ſpace my hand was in.

At this inſtant, my colleague, having his hand on the abdomen, ſaid, " Here is the placenta; but I do not feel your hand." I then applied my left hand to the place he pointed out, where I found the contracted uterus, juſt above the oſſa pubis. My right was then much higher on the placenta, which I brought away.

I introduced the hand a third time, now to examine the uterus; the orifice of which I found contracted full as much as uſual ſome hours after delivery. I paſſed up behind it, and could not doubt of my palm being on the external poſterior ſurface of that viſcus. But, to be convinced that an exceedingly dilated vagina did not occaſion deception, I turned to the woman's left, to feel for the ovarium, and got an appendage of the uterus immediately between my fingers and thumb, probably the ligamentum ovarii. I felt no coagula nor inteſtines, but did not particularly ſearch for them.

The uterus, which felt very looſe, moveable, and detached, I now reduced to its natural ſituation, and applied a bandage. I then told my colleague, that I was now ſatisfied of his having conjectured right, (for I had all along entertained ſome doubt).

He replied, that it was no conjecture; for he had felt, and even ſeen, the inteſtines of the mother.

Putreſcency of the fluids diſcharged into the cavity of the abdomen, with its conſequences, fever, and colliquative diarrhœa, were apprehended; for which reaſon, a large ſyringe, mild antiſeptic injections, alſo a flexible canula of wire covered with plaſter, were kept in readineſs. But there never was occaſion to uſe either.

The belly was conſiderably ſwelled the 3d and 4th days; with ſome, but not frequent, vomiting, and a few apthæ. Very little lochial diſcharge took place, and that not at all fœtid. No more fever than uſual in ordinary caſes. No conſiderable diarrhœa, nor any uncommon ſymptom. She had her belly kept open; took early and liberally of extract of Peruvian bark, and alſo Riverius' mixture: ſhe had in general appetite for food, and ſlept well.

I examined in the vagina, at two different times, during the firſt days, to diſcover if the vomiting had forced any thing down into the pelvis, but found nothing.

The 3d of Auguſt, the 34th after delivery, I examined again in the vagina. The os uteri admitted the point of a finger, but did not in the leaſt project. I felt no other perforation. From the back-part of the orifice of the uterus, or what I took for ſuch, a cicatrix extended about two inches obliquely downwards, and to the left of the pelvis; then ſomewhat upwards, in a ſemicircular form. I paſſed my finger ſeveral times along this cicatrix, preſſure upon which gave no pain.

Mrs Granan now walked about the houſe, alſo in the garden, and made no manner of complaint. I therefore ſuppoſe her perfectly recovered.

I have been tediouſly minute in the above narration, that every one may form a judgement of the caſe according to his own conviction. Many phyſicians of this place entertained doubts whether there had really been a laceration.

The phenomena ſucceeding a laceration of the uterus or vagina, and eſcape of the fœtus into the cavity of the lower belly, cannot fail to be exceedingly various; according as there may be larger or ſmaller blood-veſſels divided; according as any part of the child preſſes upon, or diſturbs the functions of a particular viſcus; the length of time this irritation has laſted; the ſtrength, the weakneſs, the irritability of the woman; and the impreſſion it makes upon her mind. It is likewiſe evident, that its happening from external violence only or the vehement, perhaps unſeaſonable, exertions of the woman, while ſhe is yet vigorous and healthy; or its happening after long labour, when the woman is exhauſted, the juices degenerated, the parts bruiſed, inflamed, and tending to gangrene, from the long preſſure, and repeated urgings of the preſenting part againſt the collum uteri or vagina, or the irregular projection of a limb, or other part, againſt any portion of the uterus, previouſly weakened, or rendered thin by ſuch projection, muſt make a difference.

Accordingly, on conſulting authors, we find, that ſome women have felt little inconvenience, have eaten, ſlept, and lived ſeveral days, without much ſuffering; dying, perhaps, from the putrefaction of the maſs, which was neglected to be taken from them.

Others have ſoon been affected with convulſions, vomiting, colick pains, anxiety, faintings, cold ſweats, with inability to lie in a horizontal poſture, and have died within a few hours, or have lingered in great miſery for ſeveral days.

The accident too often has not been diſcovered till after death, by diſſection. Or, if diſcovered early, yet the unhappy women were abandoned to their fate, and that even in hoſpitals of great renown. I do not mean to inſinuate, that the caſe will not in general prove fatal; a concurrence of too many lucky circumſtances are requiſite to a happy termination. I am alſo aware, that it will, in many inſtances, be difficult, in ſome impoſſible, to determine with certainty that a laceration has taken place.

Where the preſenting part recedes, and the figure of the belly undergoes a conſiderable change, with a ceſſation of labour-pains, of the motion of the child, and in ſome caſes with a contraction of the os uteri; the thought might occur to a perſon who has heard of, or experienced ſuch accidents; eſpecially if extreme debility, and other threatening ſymptoms enſue; and the introduction of the hand will determine the matter. But, where the preſentation is not changed, ſuppoſe the head locked in, or preſenting at the brim of the pelvis, the body in the cavity of the abdomen, preſſed through a rent of the uterus, now contracted, and containing the head only; or, what has oftener happened, the arm continuing in the vagina, or out of the os externum; and the lower extremities, or moſt, or the whole of the body, in the cavity of the abdomen; then indeed the ſagacity and dexterity of the beſt practitioner would be tried, and might be humbled.

Although a rupture of the uterus and expulſion of the fœtus into the cavity of the lower belly, before or after the os uteri has been dilated by labour-pains, even when left to itſelf, is not abſolutely mortal, as appears by ſeveral obſervations; yet the ſucceſsful terminations are proportionably ſo few, and thoſe after ſo much ſuffering from putrid fever, diarrhæa, abſceſſes in the belly, ulceration and perforation of the inteſtines, that they do not warrant delaying the delivery, if poſſible by the natural paſſages;

if not, by Gaſtrotomy. And the ſooner this is undertaken, the better chance there will be of ſaving mother and child. How long life may ſubſiſt in the fœtus, deprived of all communication with the mother, from a total ſeparation of the placenta, or compreſſion of the funis umbilicalis, without the benefit of reſpiration, is not yet aſcertained: but, notwithſtanding ſome rare examples, and ſome experiments to the contrary, there is reaſon to apprehend, that in general, under ſuch circumſtances, it cannot long ſurvive.

The meaſures to be purſued for effecting delivery, muſt depend on the circumſtances of the particular caſe. According as the whole, or only a part, and what part of the fœtus has eſcaped into the abdominal cavity; the ſpace of time elapſed ſince the laceration; the rupture being in the vagina, or more contractile uterus; the ſtate of this viſcus; the dimenſions of the pelvis; the knowledge, ingenuity, dexterity, and intrepidity of the operator, muſt influence the determination.

Reſpecting that caſe of ruptured uterus, where it does not happen during parturition, but at ſome earlier period of geſtation, from falls or other external violence, the diagnoſis and practice will be attended with ſtill more difficulty. We muſt, however, do as well as we can, till more enlightened by future experience. In the preſent ſtate of our knowledge, if I could determine with certainty, that ſuch an accident had taken place, and that the fœtus was expelled into the cavity of the abdomen, I ſhould not be inclined, unleſs in the early months of pregnancy, to leave it to Nature. There remains, then, only two expedients, the forcing a way through the natural paſſages, or exciſion. The former, if ever poſſible, is an arduous undertaking, will require a great deal of judgement to determine in what particular circumſtances it may be practicable or eligible, and not a little dexterity in the execution. By the other method, delivery may always, with certainty, be effected; and that method has been attended with ſuccefs.

CASE II.

ANOTHER LACERATION IN THE SAME FEMALE.

MRS GRANAN, the ſubject of the foregoing obſervations, acquainted me, in the beginning of May 1787, that ſhe was again pregnant, and near her term. As the lady propoſed lying in at her houſe in the country, ſhe requeſted me, that if it ſhould happen by day, I would come to her aſſiſtance. I took that opportunity to examine the vagina, which was uniformly dilated, without any particular contraction or ſtructure; but the cicatrix was eaſily diſtinguiſhable, and ſeemed to be harder, more extenſive, and to run more tranſverſely than it did five or ſix weeks after her delivery. She had long been ſubject to ulcers on the leg, and during the laſt eight days previous to her falling in labour, coughed much. The two laſt days ſhe was feveriſh, with an eryſipelas on one leg.

The 25th of May, Mrs Granan was ſeized with labour-pains; they were ſtrong and frequent. At ten o'clock, the membranes broke, whilſt the midwife (the ſame who had attended in 1785) was examining; whereupon ſhe made ſome attempts to alter the poſition of the head. The pains continued yet for ſome time, but weaker. By twelve o'clock they ceaſed entirely.

When I viſited the patient, at half paſt five in the morning of the 26th, ſhe was perfectly eaſy. No bad ſymptoms; the child's head in the pelvis; the ſagittal ſuture tranſverſe, lambdoidal to the left. The right ear could juſt be felt at the upper part of the ſymphiſis pubis. The tumour of the abdomen had nearly the uſual appearance. On applying a hand, the thigh, knee, and leg of the fœtus, could be diſtinctly traced; and the ſide of its body felt, in a direction between the ſcorbiculum cordis and oſſa pubis. Between theſe bones and the left ilium, a roundiſh body was felt, which I could not determine to be any part of the child; but I did not examine very minutely, nor took any alarm. No change in the poſition of the child had taken place; no particular ſenſation obſerved; and no complaint, except of ſome anxiety, which was nothing unuſual to that lady.

I waited ſome hours in expectation of pains returning. None followed, nor had ſhe felt any motion of the child ſince the preceding day. A clyſter was now given, which procured a ſtool, but excited no pains. She continued well, and took ſome food. At four o'clock, ſeeing the pains were not likely to return, I examined more accurately. The head appeared to have deſcended a little, perhaps from coughing, and from the efforts to make urine, and go to ſtool. On paſſing my finger behind the head, I diſcovered the lower edge of a laceration ; and therefore ſuppoſed the cicatrix again opened. That the action of the uterus might not increaſe the diviſion of the parts, the forceps was applied, by which the head was eaſily extracted, the occiput gradually emerging from under the pubis. But when the perinæum was ſlipped over the face, as far as the mouth, Nature made no effort, as uſual, to finiſh the expulſion of the head. I was therefore obliged to free it entirely.

After pauſing a little, finding that the child did not breathe, I proceeded to extract the breaſt, without waiting for pains, by pulling gently at the head. The face turned to the right of the mother, that it might correſpond with the poſition of the body. The ſhoulders, however, not advancing, I put a finger into the axilla, next the perinæum; brought the ſhoulder out, and the arm down. The body not ſtill coming forward, I put a finger into the axilla, under the pubis, brought it along, and finiſhed the extraction.

The lips were livid; no pulſation in the funis. Some means were, notwithſtanding, uſed to excite the vital actions, till it was perceived that the cuticle, by gentle ſcratching, was eaſily ſtripped off from ſundry places.

On applying a hand to the woman's belly, I felt the uterus of the natural ſize. I waited ſome time for after-pains: none coming, I examined with a finger in the vagina, where I found a looſe ſubſtance, which I took for the edge of the placenta, covered with the membranes; but on examining more narrowly, I diſcovered it to be inteſtines.

I now introduced the hand along the funis, till I came to what ſeemed to be a knot upon it. This, I then thought, accounted for the child's death. I could not trace the funis farther than this knot, when it ſeemed to be entangled with other parts. At a little diſtance from this, I felt ſomewhat like the funis again, running in a tranſverſe direction, round which I paſſed my finger, and found that there was no meſenteric attachment, therefore no inteſtine. I ſuppoſed then, that the funis divided, and was inſerted into two different parts of the placenta. Not comprehending what I felt, I paſſed my hand farther on, turning the palm forwards, and found a body which I at firſt thought might be the placenta. Examining whether it was every where detached, I obſerved that it was too ſmooth and uniform for the placenta; I concluded it, therefore, to be the uterus. Advancing my hand ſtill farther, I found there was nothing between the points of my fingers within, and the hand without, but the parietes abdominis,

nis. I had the palm of my hand, therefore, on the external poſterior ſurface of the uterus. I now withdrew my hand, keeping it in contact with that viſcus, till I came to the os uteri, which was more contracted than is uſual ſo ſoon after the birth, unleſs by a temporary ſpaſm. In that contracted ſtate, I could not reach far into the uterus; nor did I feel any part of the ſecundines, nor the funis leading to them. I therefore withdrew my hand entirely.

After letting the woman repoſe for ſome time, I introduced the hand again. The inteſtines were in the way; but, tracing the funis to the knot, and where it ſeemed to be entangled, by gently pulling, it ſeemed to ſlip off from its former attachment at that place, and now led me to the placenta looſe among the inteſtines.

When the placenta was extracted, I only caſt my eye haſtily over it, having many other things to attend to. I obſerved, however, that there was no knot, no branching of the funis, nor could I conceive what I had taken for ſuch. I thought of the appendix cœci vermiformis, of præternatural adheſions or indurations from the former accident, but could not form any ſatisfactory conjecture.

There had been all along no diſcharge of blood, nor did any thing like a recent effuſion follow the placenta, only a bloody ſerum, and that in no great quantity. I had felt no coagulum in the abdomen. The belly appeared to be larger than after the former birth, probably from inflation of the inteſtines; partly, perhaps, from her having lain with the ſhoulders low the former time, as it was a turning caſe; and this may, in ſome meaſure, account for the inteſtines having been leſs embarraſſing after the former birth.

I now applied a bandage very looſely, and placed her in bed, with the ſhoulders low, deſiring her not to ſit up, or make any effort to raiſe or turn herſelf. She had no bad ſymptoms ariſing from parturition. She complained only of the cough and a ſwelled eryſipelatous leg.

I left this woman at eight o'clock in the evening; and, in riding home, reflecting on all that had paſſed, began to think, that my confidence in the firmneſs of the cicatrix, the well-formed pelvis, the favourable poſition of the child, and the abſence of bad ſymptoms, had lulled me into an unſuſpecting ſecurity, and rendered me leſs attentive than I ought to have been. Even after four o'clock, when I had felt the rent, I ſtill imagined the child to be contained in the uterus, and proceeded ſlowly to extract, ſuppoſing atony only. But now, conſidering that there had not been, during the laſt eighteen hours, the leaſt pain or inclination to bear down, the feeling the parts of the child ſo diſtinctly through the abdominal muſcles, the unuſual manner of the head and ſhoulders coming away, as if extracted from a perſon who had been ſome time dead; the want of after-pains, or any appearance of hæmorrhage; I believe there is little reaſon to doubt of the child's having been expelled from the uterus before twelve o'clock the preceding night; that what I mentioned above to have felt betwixt the pubes and ileum, was the contracted uterus; and that it is probable the placenta had alſo been early expelled.

27th May. Mrs Granan has had a good night. Complains only of the cough. Almoſt no lochial diſcharge. Belly ſoft; not more enlarged. Is deſirous of food.

28th. Nearly in the ſame condition as yeſterday. A clyſter was injected, which had no effect. As ſhe had raiſed herſelf up, expecting an evacuation, I examined whether any part of the inteſtines was fallen down into the pelvis, but found nothing unuſual. I imagined that I felt the os uteri, but was unwilling to riſk doing harm, by a minute inveſtigation. Being aſked, if I would look at the after-birth again, was glad to learn that it was not yet deſtroyed. The ſituation it happened now to lie in, explained ſatisfactorily to me the deceptive feel of a knot upon, and forked branching of the funis. A portion of the membranes had made a turn round the funis, (moſt likely the placenta had made a rotation), not far from its inſertion into the placenta. The extremity of this portion of the membranes formed a pouch, containing a large coagulum; the weight of which gave the membranes, ſtretched between the coagulum and funis, a conſiderable degree of tenſion, the reſemblance of a cord; and, in paſſing round the funis, the feel of a knot, or entanglement.

Although Mrs Granan had not, after this birth, any very threatening ſymptoms, yet ſhe ſuffered much more than after the former accident. She was, from the 29th of May, for a couple of weeks, more or leſs feveriſh, owing partly to reſorption, partly to ſuppuration which was going on in the abdomen. She had diarrhæa, aphthæ, and ſometimes vomited, but had withal, in general, ſome appetite for food. The lochial diſcharge was upon the whole ſcanty, the firſt eight days fœtid. I find noted of the 3d June, that the belly was leſſened in ſize, but hard; the 11th of June, that ſhe was feveriſh, the belly enlarged, with a tympanitic feel. The 18th of June, ſhe was pretty well; the abdominal tumor reduced. An abſceſs pointed near the navel; but, being miſtaken for a hernia, was not opened till I ſaw her on the 24th, when about ſix ounces of thick fœtid matter were ſlowly diſcharged from it. The entrance into the abſceſs had

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a winding courſe, which prevented a probe or bougie penetrating; but, four weeks afterwards, when it became neceſſary to dilate the ſinus, a probe penetrated about four inches, in a direction towards the pelvis. Mrs Granan, who dreſſed the ſore herſelf, kept the ſinus open, by introducing occaſionally bits of baſs violin-ſtrings, or prepared ſponge. The 17th July, when I ſaw the patient for the laſt time, the abſceſs was not quite ſhut; but put her to no inconvenience, nor hindered her from viſiting her friends.

Reſpecting the alterations in the ſtate of the uterus and vagina, I find noted in my journal: "6th of June, on examining per vaginam, found in the bottom of it a round hole, without any projection, like what I had taken for the orifice of the uterus, the 34th day after the former delivery. To the right of this, an aperture about two inches in length, which I took to be the remains of the laceration. Examining all round the edges of this aperture, gave no pain. I could not diſtinguiſh any thing beyond it.

18th June. What I took for the orifice

of the uterus has nearly the ſame feel as above obſerved, the 6th. What ſeemed to be the rent is now about three quarters of an inch long.

7th July. Six weeks after the accident, the orifice in the bottom of the vagina, which I took for that of the uterus, juſt admitted the tip of a finger. What I had taken for the laceration to the right of the orifice juſt mentioned, was ſtill open, but ſo much reduced as only to admit the point of a finger. Touching it gave no pain. The old cicatrix (to the left, reſpecting the pelvis) felt juſt as it did five weeks after the former birth. I ſuppoſe that to the right was a new laceration.

17th July. I found what I had taken for the os uteri, rather more contracted. The laceration juſt diſtinguiſhable by the point of the finger; perhaps a bougie would ſtill have paſſed through it.

It was not poſſible for me to viſit this patient daily; nor was it neceſſary, as ſhe was under the care of an able phyſician, Dr Schutt, who attended her alſo during the former lying-in, and merited both her and my entire confidence.

I wiſh that I had attended the whole progreſs of this laſt labour from the beginning, and could have obſerved whether the ſuppoſed orifice of the womb dilated, or what other part gave way. It is mentioned above, that I found the orifice of the uterus, the 6th of June, in the like ſtate as I had felt it the 34th day after the former delivery. This is a degree of contraction not to be expected in the ſpace of twelve days, and renders it doubtful whether the child came out through that orifice, or forced a new paſſage through the collum uteri. At the ſame time, I believe that the orifice mentioned was really that of the uterus, or led to it; becauſe, after the former recovery, I found no other perforation in the vagina, for the diſcharge of the menſes, or to admit of impregnation.

Taking the abſceſs and other ſymptoms into conſideration, compared with what I obſerved on diſſecting the ſubject of my fourth caſe, I am inclined to think, that the laceration this laſt time extended to the body of the uterus. I always ſuſpected, that the midwife (an ignorant, conceited, and enterpriſing ruſtic) perforated the vagina the firſt time, in endeavouring to reduce the hand; and did miſchief the laſt time, by her attempts to alter the poſition of the head.

CASE III.

UTERUS BURST DURING PARTURITION.

I WAS ſent for, Saturday the 29th of Auguſt 1786, to a woman under middle age, who had been in labour of a firſt child ſince Wedneſday the 26th in the afternoon. The midwife ſaid, the waters had preſented in a large form, and broke of themſelves on the Thurſday morning; that the pains had little effect. Friday, the woma vomited often. The night between Friday and Saturday, ſhe had not ſlept. Saturday morning at nine o'clock, ſhe heard a noiſe, as if ſomething had burſt within her. Since that time, till I ſaw her about eleven of the forenoon, ſhe had not had any labour-pains, but much anxiety and faintneſs; the pulſe ſcarcely to be felt; her hands were cold. The belly was very prominent; the midwife ſaid, after hearing the noiſe, it was become fuller in the upper part; and that the head had changed its poſition in the vagina. But ſhe could not deſcribe the change. It had, perhaps, receded a little. I found the head, however, yet low enough for the application of the forceps. The ſcalp felt looſe upon the cranium, as it does when putrefaction has leſſened its adheſion. The poſition of the bones reſpecting one another, and the ſharp feel of their edges at the ſutures, left no doubt of the child's having been already ſome time dead; although the woman ſaid, ſhe had lately felt it move.

The abdominal tumor was ſtill perfectly circumſcribed, as if the child and waters were yet contained in the uterus. The moſt prominent part of the much-diſtended belly, had an unuſual ſoft feel. I, at firſt, apprehended that the urinary bladder was ſtretched before the uterus; but, by repeating the examination, was convinced of its being air. There was a diſagreeable cadaverous ſmell in the chamber.

Although I had no proſpect of doing any good here, yet I would not abandon the woman entirely; as it might, poſſibly, be only a temporary faintneſs. I therefore went home for my inſtruments; but, when I returned after about a quarter of an hour, the woman was breathing her laſt; and expired in about two minutes. I propoſed opening her immediately, to take out the child; perhaps to ſave a ſecond child, of which the ſize of the belly and œdematous extremities gave ſome ſuſpicion. Beſides, I was very deſirous of examining into the cauſe of her death. But this the huſband could not be prevailed upon to permit. He, however, bethought himſelf, about ſix hours after, and ſent to acquaint me, that I might do as I thought proper. The belly being then much more elevated, the fœtor conſiderable, and the weather very hot, I heſitated a little, on account of the putridity. But the deſire of inveſtigation prevailing, I made a ſhort inciſion through the containing parts, where thinneſt; intending, ſhould the ſmell prove very offenſive, to ſew it up again immediately. A ſmall quantity of fœtid air iſſued at the opening, but not very intolerable. I therefore proceeded to expoſe the uterus fully. There were only ſix or eight ounces of bloody ſerum in the abdomen; no conſiderable fœtor, nor diſeaſed appearance; except that the uterus was exceedingly diſtended, and of an uniform figure, as if blown up. To the left, it was emphyſematous; the external tunic was there ſeparated from the ſubſtance of the uterus. Under the ligamentum latum of that ſide, an arm of the child could be felt, covered only by the peritonæum. I now opened the uterus; which was not thicker than the eighth part of an inch, and tore as eaſily as a ſheet of writing paper, eſpecially towards the left. It contained no detached air. The child was exceedingly emphyſematous; the nates uppermoſt, and forwards; the feet backwards, inclining to the left. It required a conſiderable degree of force, to withdraw the head from the pelvis. The placenta was eaſily ſeparated. No appearance of large veſſels or ſinuſes; no extravaſation; no coagula. But, now that the uterus was emptied of the child and ſecundines, there ſoon accumulated a great deal of bloody ſerum in it; which was removed by a ſponge, but continued to accumulate quickly. Searching for the ſource of this fluid, I found that it had been collected under the peritonæum, from where it covers the muſculus iliacus internus, to above the kidney, and back as far as the ſpine; but there was no coagulum. The laceration in in the uterus began backwards, a little above the os tincæ; extending to where it was mentioned, that the arm of the fœtus was felt under the ligamentum latum.

Was not the uterus burſt, in this caſe, by the increaſing bulk of the child? Was not procraſtination here carried too far? What muſt be the conſequence of attempting to turn a child, under the like circumſtances*?

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* The uterus, in this woman, being ſo exceſſively diſtended, could exert but little power towards

CASE IV.—RUPTURED UTERUS.

ON the 1ſt of February 1789, I was ſent for to a woman in labour of her ninth child. She was about thirty years old. Had twice born twins. All were natural births, and living children. I was further informed, that this labour had commenced the preceding day, at two o'clock in the morning. At three, the waters were diſcharged copiouſly. Trifling pains continued the whole day; but, from eleven to twelve at night, the pains had been ſtrong, as the attendants

ſaid;

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wards expelling its contents. It was even in a much worſe ſituation than it is, when overſtretched by a plurality of children, or great quantity of liquor amnii; becauſe, in the laſt caſe, the diſtenſion happening gradually, the uterus thickens in proportion, and the diſcharge of the waters, or the birth of a child, gives an opportunity for its recovering more force and contractility. Whereas, in the firſt caſe, the extraordinary diſtenſion took place ſuddenly; and was, after the diſcharge of the waters, hourly increaſed by the putrefaction.

ſaid; which, however, the midwife contradicted. All agreed, that, after midnight, the woman had not had any forcing pains; but only complained of anxiety, and a continual pain in the belly, particularly to the left. At half paſt three in the morning, ſhe had convulſions, with ſome diſcharge of blood from the vagina.

When I came there, at ſeven in the morning, the woman was ſitting up in bed; not being able to lie down, on account of anxiety. Her pulſe was very weak; the hands not cold. The obſcurity of the chamber prevented me from making any obſervations on her face. The vagina was filled up with a large hemiſpherical body, forwards; and behind, with a tranſverſe fold, about the ſize of a child's arm. But both were ſoft, and evidently connected with the pelvis. I could, however, feel no part of the child.

Mentioning this laſt circumſtance to the midwife, ſhe affirmed having felt the head. I hinted, that as the upper part of the ſacrum projected much forwards, ſhe might have taken it for the head. The midwife replied, that ſhe had introduced her hand, and paſſed it quite round the head; but, ſince the convulſions, ſhe had felt nothing of it.

Suſpecting that the uterus was ruptured, and that the child was already expelled into the cavity of the abdomen, I examined the belly attentively. It had, however, the ſame uniform feel, and roundiſh figure, as when the fœtus with the waters are included in the uterus; only, at two particular places, the feel of ſome ſmall parts of the child ſeemed to be too ſharp and diſtinct.

I now took her huſband and mother into a ſeparate apartment; told them my opinion of the caſe, that there was very little chance of recovery; that there was not an abſolute certainty of the child being dead; and that I would attempt to deliver the woman, if they deſired it; but that they muſt not be ſurpriſed, if ſhe died during the operation.

Delivery being reſolved on, I had her properly placed; introduced my hand; felt the placenta looſe at the brim of the pelvis. Beyond that, the child's head and a hand.

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A little higher, a foot; which I brought down; but, the child not eaſily following, I put a nooſe on the foot; and, along that leg, went up in ſearch of the other foot, being afraid it might have ſuch a poſition as to increaſe the laceration, or injure the contents of the abdomen, if in that cavity, of which we were not yet certain. Having now both feet, I proceeded to extract the child, which was of a very large ſize. It required conſiderable force to bring the breaſt through the pelvis, and the head through the upper ſtrait. I have, however, ſeen children born alive, where equal force and more time were required to extract them. But this was dead.

A conſiderable quantity of bloody ſerum guſhed out with force, whilſt I was bringing down the feet; and, during the extraction of the body, urine flowed from the urethra.

The ſecundines being now alſo immediately brought away, I introduced my hand again, to aſcertain, whether the uterus was really ruptured; and, for the firſt time, felt it at the brim of the pelvis; hard and contracted, ſo as to admit with difficulty the points of three fingers. This made it probable, that the uterus had been for a conſiderable time empty; and that I had not extracted the child out of, or through it; and that the laceration muſt be in the vagina. Paſſing my hand, to the right of the uterus, gradually higher, I met with no reſiſtance, till I came to the firm ſmooth ſurface of the parietes abdominis, forwards; and backwards, I felt the inteſtines.

The woman made no manner of complaint during this inveſtigation. However, I did not think myſelf at liberty to carry reſearch farther on a living ſubject.

A bandage being now applied, the woman was put to bed. She bore the operation well; nor did her ſtrength diminiſh. A light cordial was given frequently; but ſhe died about five hours after delivery.

Although I had no doubt of there being a laceration, and that I had extracted the fœtus and ſecundines from the cavity of the abdomen; yet I wiſhed to know the whole extent of the injury; and offered to finiſh my examination in the ſpace of three minutes; but could not, by any means, prevail upon the relations to allow the body's being opened. I got, however, an opportunity by night, after the body was ſent out of the houſe for interment, to examine it haſtily in the coffin.

The belly being uncovered, appeared flat and livid. The parietes of the abdomen made little reſiſtance to the knife, or to a tearing force. The recti muſcles, I expected, as ſhe twice carried twins, to have found ſeparated from one another to a conſiderable diſtance; but they were not, nor was the belly in the leaſt pendulous. The interſtices between the turns of the inteſtines were filled with dark-coloured bloody ſerum, of the conſiſtence of liquid blood. The ſurface of all the contained parts were florid. The uterus appeared to be entire, and of a conſiderable ſize, extending from the oſſa pubis to the tranverſe flexure of the colon, which divided the cavity of the abdomen into two nearly equal parts. Between the uterus and that inteſtine, lay the omentum, irregularly, but cloſely folded up; it was, however, eaſily extended, ſo as to reach the pubis. The inteſtines, almoſt empty, had no tendency towards the pelvis; which was otherwiſe ſecured againſt their intruſion, by the uterus. The fluid blood, which amounted to ſix or eight ounces, being removed, and the uterus drawn well up towards the diaphragm, the peritonæal covering, now detached from the whole anterior ſurface of the collum, and half-way up to the ſuperior part of the fundus uteri, did not follow. When that membrane was alſo ſeparately drawn upwards, it formed a pouch, with a large tranſverſe aperture. This aperture or rent in the peritonæum, was bounded to the right by the beginning of the round ligament; its termination to the left, I do not recollect. There was, moreover, a laceration in the ſubſtance of the uterus; which did not, however, correſpond with that of the peritonæum; but began at or lower than the oſſa pubis, and extended upwards obliquely to the right; terminating near the origin of the round ligament, and of equal height with the ovarium. I have ſaid that the laceration began at the oſſa pubis; but where it originated, reſpecting the ſoft parts, I could not preciſely determine; as, from the little time allowed me, the contuſion and deſtruction of parts, I could not diſtinguiſh what was collum uteri, or what vagina, anteriorly. But a large projection of the os tincæ, ſtill very low in the pelvis, even whilſt the uterus was drawn upwards, evidently ſhewed where the collum uteri terminated backwards. This circumſtance, with the low ſituation of the bladder, which formed the principal bulk of the prolapſus forward, renders it probable, that the anterior portion of the vagina was wholly ſunk in the pelvis, and that the laceration was entirely or principally in the collum and body of the uterus. The opening into the cavity of the fundus, at the upper part of the laceration, would now have eaſily admitted the flattened hand: there muſt then have been a temporary ſpaſmodic conſtriction at the moment, which I had taken for the contracted os uteri. The edges of the laceration, where thinneſt, might be a quarter

ter of an inch thick, or upwards; but had a ſpongy feel; and inciſions into them, ſhewed that the thickneſs was partly owing to extravaſated blood, perhaps from long continued contuſion. When the uterus was opened, by a perpendicular inciſion, to the left of the rent, its ſubſtance had a whitiſh appearance. The anterior part of the fundus was full two inches thick *.

There

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* The great diſproportion (viz. eight to one) in the thickneſs of the upper part of the uterus, compared with the middle and lower parts, which I have termed *body* and *collum*, cannot be ſuppoſed to have exiſted in that degree, during pregnancy; but was owing no doubt, in a great meaſure, to the action of that organ during parturition. The efforts of the fundus, or upper part, overcoming the reſiſtance of the lower parts, forced its contents into them, dilated and rendered them thinner; the upper part at the ſame time acquiring more thickneſs and ſolidity. But the contents of the uterus, which in this woman found no exit through the pelvis, would accumulate in the lower portion of that viſcus, till the part moſt ſtrained or bruiſed muſt give way. This ſtate of the parts may account for my having miſtaken the upper firmer part for the whole

of

There were no coagula, except a ſmall dark-coloured one in the vagina. The ſpace from the ſymphyſis pubis to the upper part of the ſacrum, meaſured exactly three and a half inches Engliſh.

The ſtupidity and inattention of the people about this woman, made it impoſſible for me to obtain a ſatisfactory account of what happened, or of what ſhe complained during the labour, which renders this caſe leſs inſtructive than might be wiſhed.

The pains were reported to have been inconſiderable during the whole time of labour. This was, perhaps, reſpecting their effects in promoting the birth; or, perhaps, the woman was uncommonly patient, for it muſt be ſuppoſed that ſhe had ſuffered excruciating pain. And, in fact, ſome time after I had noted down the whole of the above, the midwife, in conſequence of ſome queries I had put to her, informed me, that on

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of the uterus; the contracted entrance into its remaining cavity for the natural orifice; and for my not diſtinguiſhing the looſer, ſofter, lower portion, from the other ſoft parts.

on pondering the ſubject in her mind, ſhe recollected, that the pains were, in every ſenſe, weak in the forenoon; but, at four afternoon, the woman began to complain that the pains, which took their riſe in her back, went round the haunches, and ended in her belly, as if the child would force its way out there. She complained alſo of cutting pains in her belly; and the midwife ſaid, that ſhe had herſelf felt the child urging forward that way; that, juſt before the convulſions happened, the woman had cried out, "Oh! my heart, my heart will come out of my mouth." The midwife farther informed me, that the former children were all ſmall, and that the waters had remained to the laſt.

It is doubtful whether the laceration took place at twelve or half-paſt three, or by a gradual progreſs; whether the convulſions were the cauſe or conſequence. Perhaps the pain of beginning laceration, or the internal hæmorrhage, occaſioned the convulſions, and theſe completed the expulſion of the uterine contents.

I have no reaſon to ſuſpect that the midwife

wife did material injury; for, in replying to my queſtion, ſhe ſaid, that ſhe had not introduced the hand in order to aſſiſt, or to alter the poſition, but merely to examine if any other part preſented with the head. Nor did ſhe pull at the parts filling up the vagina, nor took them for any part of the child, knowing that the woman had a prolapſus. Had the rent been in the vagina only, the midwife might have occaſioned it, by rudely going up in a wrong direction. There probably was prediſpoſition to rupture, a great inequality in the thickneſs and ſtrength of the propelling viſcus, irregular uterine contractions, an unfavourable poſition of the head, which otherwiſe was evidently diſproportioned to the capacity of the pelvis. The huſband ſaid, that this child was much larger than any of the former; that all the former labours had been ſlow, requiring at leaſt thirty-ſix hours.

Though the prolapſus, including the more or leſs diſtended bladder, did not make any conſiderable reſiſtance to the deſcent of the head; yet, the bladder being ſunk below the upper edge of the oſſa pubis, there remained ſo much the leſs ſoft medium between the uterus and theſe bones; the collum uteri would therefore be more expoſed to injury from the urgings of the child's head; and the waters breaking ſo early alſo increaſed the difficulty. Perhaps not any one of thoſe cauſes would, ſingly, have produced an effect to which, in conjunction, they were equal.

The fœtus remaining convoluted, and retaining the roundiſh form in the abdomen, the head likewiſe being ſtill the loweſt part, renders it probable that it was not alive at the period of expulſion from the uterus. It puzzled me exceedingly, to conceive how, with that form and poſition, the child could be expelled from the uterus through a rent in the vagina; or from that part being partially torn off from the uterus, which, before diſſection, I imagined to be the caſe. But, as the laceration extended high in the body of the uterus, and that in an oblique direction, which is in a leſſer degree tranſverſe, it is not difficult to imagine, that the containing and expelling viſcus, being once detached from one ſide of the pelvis, would, by its contractions, be gradually drawn over towards the fixed points to which it remained connected, at the ſame time forcing out its contents, *qua data porta*; and that a laceration once begun, would be eaſily extended to any requiſite degree.

From the peritonæal covering of the uterus being ſeparated to ſuch a great extent, and not torn oppoſite to, or in the direction of, the rent in the ſubſtance of the uterus; it is probable that the ſubſtance of that viſcus firſt gave way, and that a conſiderable portion of the fœtus was without the uterus, before the peritonæal tunic burſt tranſverſely at the uppermoſt part of the rent in the uterus.

From the large ſize of the child's head, its being no way flattened or diſtorted, the abſence of ſwelling or diſcolouration of the integuments, as well as from the midwife eaſily paſſing her hand round it, I ſuppoſe it never had been in the pelvis.

As the child lay in the right ſide of the abdomen, the emptied uterus would of conſequence be puſhed over to the left. Hence, perhaps, the pain complained of in the right ſide of the belly.

The uniform diſtenſion and roundiſh figure of the belly, notwithſtanding the waters had been early diſcharged, was owing to the great quantity of extravaſated fluids collected in the abdomen, and retained by the large portion of detached peritonæum, acting as a valve, ſupported, perhaps, by the prolapſus. This valve, when raiſed by the introduction of the hand, and whilſt the feet were in the pelvis, opened a paſſage for thoſe pent-up fluids, which were then diſcharged with impetus. The urine was alſo forced out of the prolapſed bladder, as ſoon as the nates and body of the child filled up the pelvis.

The florid colour of the ſeveral parts contained in the abdomen, I at firſt attributed to the extravaſated blood, changed by the admiſſion of air through the laceration; but as the ſurface of the fluid occupying the interſtices of the guts, and the coagulum found in the vagina, were dark-coloured, peritonæal inflammation is rather to be conſidered as the cauſe.

II.

II.

Hiſtories of different Tetanic Complaints, in which the moſt powerful remedies were employed in vain. By Mr Dallas, *Surgeon on board his Majeſty's ſhip the Union. Communicated in a letter to* Dr Duncan, *from the iſland of St Vincent's.*

DURING a reſidence of four months in the iſland of Carciacow, I had occaſion to obſerve the dreadful conſequences of Tetanic complaints, (ſo frequent and fatal in this climate), and the inefficacy of the moſt celebrated antiſpaſmodics, recommended in that dreadful diſorder, even when adminiſtered under the inſpection of an ingenious and experienced practitioner, whom I at that time aſſiſted. Should you think theſe obſervations worthy a place in your uſeful *Annals*, they may perhaps be the means of leading to further inveſtigation into the nature of the diſorder; which, in my opinioin, is by no means well aſcertained; and the treatment, of courſe, imperfect.

The firſt caſe which came under my care, was that of a ſtout healthy negro-man, about twenty-five years of age, who was admitted into the Hoſpital on account of a prick which he had received from a cotton-ſtump, in the ſole of his foot. From the pain with which it was attended, I was apprehenſive there might be a portion of the wood remaining; but upon enlarging the wound, I could find none.

The ſore was waſhed with ſpirit of turpentine, and covered with a pledget of lint, ſpread with cerate. It was dreſſed in this manner for two or three days, and no further notice taken of it, the wound being ſo very trifling.

On the fourth day, however, upon inquiry, I found the pain (which had never entirely left him) was increaſed conſiderably, ſtretching upwards, with ſlight feveriſh ſymptoms.

The following pills were preſcribed:

℞. Gum. Camph. ʒj.
Calomel ppt—ʒſs.

Opii pur. Gr. xviii. M. bene, et divid.
in pil. No. xviii.

He had a clyſter to clear his bowels, and was ordered one of the pills three times a-day.

Next morning, I found no alteration for the better; he had violent pain from the wound upwards, which totally prevented his getting any ſleep. Pulſe natural, and bowels regular. The pills were ordered to be continued, and a cataplaſm with opium to be applied to the ſore, which looked whitiſh, and diſcharged a thin ſerous matter.

About four in the evening, I was informed he complained of a pain in his back, and a ſtiffneſs in the muſcles which ſerve to draw the head backwards. The pain of the leg ſtill continued. He had had no ſtool ſince the morning; a clyſter was immediately thrown up, which cleared his bowels. He alſo took two of the pills. Mercurial frictions were likewiſe begun upon his back and neck, in which two of the nurſes were kept conſtantly employed.

At bed-time, I found all the bad ſymptoms increaſed. He had had no reſt the two preceding days and nights; the pain of his back violent, eſpecially when he attempted to move; and ſome difficulty of deglutition. He ſwallowed three of the pills immediately, and the frictions were ordered to be continued through the night. Should there be no appearance of ſleep, the cataplaſm with the opium to be renewed to the ſore.

Upon viſiting him in the morning, I found he had paſſed a very reſtleſs night, with frequent ſpaſmodic twitches; in which his head was violently drawn backwards. He had got no ſleep, and his jaws were ſo locked, and the difficulty of deglutition ſo great, that it was with the utmoſt pain he ſwallowed his pills and a little wine.

He had had no ſtool through the night; and, notwithſtanding the almoſt continual friction, and the internal exhibition of the calomel, there was not the ſmalleſt affection of the ſalivary glands, which I eagerly looked for. I ſtill, however, perſevered in my firſt plan, and directed the mercurial frictions and pills to be continued as often as he could ſwallow them.

Towards noon, the ſpaſms got ſo frequent, and the difficulty of deglutition ſo great, that every attempt to ſwallow his pills, or any nouriſhment, was ſure to bring on a convulſion of his whole frame; in one of which he expired about four in the evening.

As no candid practitioner would form a decided opinion of the effects of any remedy upon its ſucceſs or failure in one ſolitary caſe, I was determined to give the mercury and opium a further trial.

On Monday, the 6th of February 1796, I was called to viſit a ſtout negro-man, about twenty years of age, whom I had occaſionally ſeen two or three months before, on account of a hard indolent tumour, ſituated in the upper part of the metatarſal bones of his foot. He had had an emollient cataplaſm applied to it daily, for a month before it ſuppurated, and it burſt on the Friday preceding my being called, and diſcharged a conſiderable quantity of matter.

When I ſaw him, I was informed he had been attacked with ſymptoms of locked jaw in the middle of the night, which continued increaſing, and were then pretty ſevere;

his head being frequently drawn backwards, with ſtiffneſs of his jaws. I examined the ſore upon his foot, which diſcharged a thin acrid-looking matter, and was attended with a good deal of pain ſtretching up the leg.

Dr Bartlett, who had treated ſome caſes of Tetanus in adults, where there was no wound inflicted, to which he could attribute its cauſe; and as they almoſt always occurred in January or February, when the weather is very cold, the wind then generally blowing from the northward, as it at this time did, was inclined, at firſt, to ſuppoſe that our patient's complaint was owing to the ſame cauſe, as we were certain there was no cutting inſtrument uſed by which the nerves could be in the leaſt injured. We, however, concurred in opinion at laſt, that the diſeaſe was more probably occaſioned by the acrid nature of the diſcharge; which, by irritating the tender nervous filaments, produced the ſame effect as a ſharp cutting inſtrument, though in a leſs degree. As the treatment, however, muſt have been the ſame, from whatever cauſe the complaint proceeded, and as there was no time to loſe,

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I inſtantly ordered an injection, which cleared his bowels, and brought off a quantity of hard excrement. He ſwallowed ſixty drops of liquid laudanum, and five grains of calomel; and two people were employed aſſiduouſly in rubbing into every part of his body ſtrong mercurial ointment.

From the bad ſucceſs which attended the treatment of my former patient, I was induced to increaſe the quantity of mercury and opium. He accordingly took ʒiſs of liquid laudanum, with ſix grains calomel, every hour; and the frictions were continued.

The ſymptoms, notwithſtanding, increaſed rapidly, till about five P. M. when the ſpaſms, in which his whole frame was horribly convulſed, and the difficulty of deglutition, were ſo great, that it was with the utmoſt difficulty we could prevail upon him to take his medicines. And his jaws were only prevented from being entirely locked, by a piece of wood wedged between his teeth.

About ſeven o'clock, I began injecting laudanum by the anus, and threw up at the rate of half an ounce every hour. But neither this nor the mercury had the ſmalleſt viſible effect, either in mitigating the ſpaſms, inducing ſleep, or bringing on a ſalivation. He expired about midnight.

I have ſince ſeen four caſes of Tetanus; one of which was treated in the ſame manner as my two unfortunate patients. Recourſe was had to bark and the cold bath, in the other three; but the ſame unfortunate iſſue attended the whole.

I have converſed with may eminent practitioners in this and the neighbouring iſlands upon this ſubject; and I am well convinced, that there is not one well-authenticated caſe in fifty, where the diſorder was brought on by leſion of a nerve, in which the patient recovered. I ſpeak from what I myſelf have ſeen.

III.

Hiſtory of a ſingular Caſe in Midwifery, where delivery was accompliſhed in conſequence of an Inciſion by the Vagina. By Dr Iſaac Cathrall, *Philadelphia.*

ON the 29th of March 1797, I was ſent for to Mrs J. of a good conſtitution, twenty-one years of age, in labour with her firſt child. The midwife informed me, that ſhe had had ſtrong pains for near ſix hours; and that the head was low down in the pelvis. But there was no diſcharge from the uterus; and ſhe could not diſcover the os tincæ, or any opening into the cavity of the uterus.

On examination, the circumſtances were found nearly as ſhe had related; but from the vagina projected a protuberance, about half the ſize of an egg, of a ſpongy vaſcular texture, and of a dark colour, but not pervious.

vious. This protuberance was puſhed forwards by the head of the child during every pain, in the abſence of which it receded. During both ſituations, the parts were expoſed, and minutely examined for the os tincæ and opening into the uterus; but nothing could be obſerved more like the former than what we have already deſcribed; the vagina appearing like a blind pouch.

The circumſtances of the caſe being aſcertained, after ſome reflection on the novelty of its appearance, there ſeemed to me two ways in which the delivery of the child might be accompliſhed. The firſt was from the efforts of the patient, propelling the head of the child through the uterus. The other, from cutting down through the uterus on the head of the child, and ſuffering the pains to dilate the opening, and finiſh the delivery.

The former ſeemed dangerous, and very precarious; for the woman's ſtrength was on the decline, and her efforts gradually decaying: ſo that it was uncertain, whether her powers would be adequate to the effect; and, when produced, as uncertain in what part of the uterus the rupture would happen. For, if it took place in the direction of the head of the child, it might be attended with ſucceſs; but if in any other part of the uterus, the death of the patient would, in all probability, be the conſequence. Theſe conſiderations induced me to relinquiſh the idea of leaving the caſe entirely to nature, and to give the latter mode the preference. This ſeemed not deſtitute of hazard, on account of the hæmonhage that might enſue; but there appeared no alternative, but what promiſed leſs ſucceſs.

Accordingly, the patient was placed on her back, with her legs ſeparated, and drawn up towards her buttocks. When I was preparing to make the inciſion, her connections requeſted the operation might be poſtponed until further advice could be obtained.

At four P. M. Dr Say was requeſted to viſit the patient; and, after minutely examining the parts, agreed to the operation.

We then reſumed the poſition previouſly mentioned; when I made an inciſion, while the parts projected, in conſequence of a pain, about one inch and a half in length, through the projecting body, downwards with reſpect to the peritonæum, and inwards as to the child's head. But it required ſeveral ſtrokes with the ſcalpel, before we could cut into the cavity of the uterus; the thickneſs of the projecting body being more than one inch.

The hæmorrhage which enſued, was trifling, gradually diminiſhed, and at length entirely ceaſed; and in an hour ſhe was delivered of a healthy child.

During the progreſs of the labour, we were much aſtoniſhed at finding no liquor amnii diſcharged; neither was there the leaſt appearance of this fluid immediately after the operation.

After the placenta came away, and the patient was recruited, a linen tent was introduced into the uterus; and ſhe was then put to bed.

30th. She reſted well, was free from pain; and diſcharged urine ſeveral times in the night, without much uneaſineſs.

31ſt. She continued as well as yeſterday; and diſcharged the lochia through the tent.

From this time, I was prevented from viſiting her for two months; when ſhe informed me, that ſhe recovered very ſoon from her indiſpoſition, without any further medical aſſiſtance; and was then in a good ſtate of health. I ſolicited an examination of the parts, which ſhe, with reluctance, complied with; when I found that the projecting body that was divided in the operation, had united; and poſteriorly to that, there was a ſmall opening into the uterus, which was ſurrounded by an elevated edge, bearing a diſtant reſemblance to the os tincæ after parturition, both in ſituation and appearance.

The favourable termination of the preceding caſe, manifeſtly points out the propriety of doing the operation early, under ſimilar circumſtances; and evidently ſhews, that the os tincæ, and neck of the uterus, may be freely divided, without profuſe hæmorrhagy, or ſubſequent alarming ſymptoms; for our patient recovered nearly as well as if no operation had been performed! Whereas, if the caſe had been left to nature, or the operation poſtponed until a more advanced period of the labour, from a hope of the os tincæ dilating, there would have been a probability of the woman dying, from a rupture taking place in the body of the uterus; or expiring from exhauſtion, without ever effecting the expulſion of the child.

When I firſt contemplated the circumſtances of this caſe, I did not immediately recollect, that my reading, or opportunities in practice, furniſhed me with an inſtance analagous. But, on opening Dr Smellie's Treatiſe on Midwifery, I found among the caſes one related by Dr Simpſon, Profeſſor of Medicine in the Univerſity of St Andrew's, very ſimilar in many of its ſymptoms to the one I have related. The Doctor, with the concurrence and aſſiſtance of Dr Haddow, divided the os tincæ into the cavity of the uterus; and with great difficulty effected the delivery of a child that appeared to have been ſome time dead. During this operation and the time of the labour, there was no liquor amnii diſcharged. After the patient was delivered and put to bed, ſhe was ſeized with pleuritic ſymptoms, and expired in twenty-four hours after; in the Doctor's opinion, not from the operation or its conſequences, as neither pain nor hæmorrhage enſued, but from the injury induced from the length and ſeverity of the labour. The want of waters, ſays the Doctor, in this caſe excited ſome ſurpriſe, till I recollected, that, in the time of labour, the patient told us they were paſſing. At which time, I had the curioſity to make ſtrict obſervation, and found what ſhe called the waters paſſed by the urethra, which opened externally by three diſtinct orifices. This, with her having loſt, in conſequence of inflammation, ſuch a portion of the coats of the bladder, which was diſcharged by the urethra, after a former labour, together with her being ſubject to gravel, gave me grounds, ſays the Doctor, to think that there was ſome communication between theſe paſſages and the cavity of the womb, above the os tincæ, which had allowed the waters to be evacuated. I was the more inclined to entertain this ſuppoſition, becauſe frequent inſtances have been obſerved of ſtones making their way through the neighbouring parts, as happened to a boy in this neighbourhood, who paſſed a very long ſtone which had lodged long in the bladder, by the anus*.

This conjecture of a communication between the uterus and bladder, however plauſible it may appear, in accounting for the abſence of the liquor amnii in the patient of Dr Simpſon, does not ſeem applicable in explaining a ſimilar phenomenon, in the caſe I have related. Though the ſtate of the os tincæ, and abſence of the liquor amnii, were the ſame, yet, in other reſpects, the circumſtances of our patients were diſſimilar, for mine had never born a child, and conſequently could have received no injury to her bladder from labour. Neither had ſhe been ever affected with gravel, to afford the leaſt ſhadow of probability of a communication being effected, through which the liquor amnii could have paſſed during labour; but, on the contrary, ſhe had enjoyed a good ſtate of health. Therefore

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* See Dr Smellie's Treatiſe on Midwifery, page 348.—Caſe 28.

fore the cauſe of the abſence of the liquor amnii muſt be ſought for in ſome other ſource. And, I think, it may, with more probability, be attributed to an increaſed power taking place in the abſorbents of the uterine ſyſtem, previous to, or during labour.

IV.

Account of a Contagious Fever, which appeared in his Majeſty's 88*th regiment, in the iſland of Jerſey, in July* 1797. *By Mr* James M'Gregor, *Surgeon to the regiment.*

THIS fever at firſt wore the moſt alarming appearance; and, for the laſt four years, having been a witneſs to the very ſevere loſs, which, in different parts of the world, the 88th regiment ſuffered by a contagious fever, I was induced, at its firſt appearance, to pay the cloſeſt attention to it.

The 88th regiment landed on the 6th of June 1797, in Jerſey, with 400 men, all very healthy, and for nine months preceding their arrival in the iſland, the regiment had been in the moſt healthy condition.

The firſt caſe of fever which appeared was that of Lieutenant Dixon. He was attacked on the 17th July. His diſeaſe was the

Scarlatina Anginoſa. But, of fifty-four caſes that have ſince occurred, there was only another with the ſcarlet eruption and angina.

There was no remiſſion in Lieutenant Dixon's caſe till the ninth day; and before this time, he had the worſt and moſt malignant ſymptoms. He is now, however, in a ſtate of convaleſcence.

On the night of the 19th July, Skinner, a venereal patient in the hoſpital, was attacked; and on the morning of the 23d, died of low Typhus.

Several men were complaining of ſymptoms of fevers at the time of Skinner's death; and ſome of them being of the company to which he belonged, fears were entertained of the fever being contagious. I determined, therefore, immediately to ſeparate every one, complaining of any ſymptoms of fever, from thoſe in health; and for this purpoſe had ſeveral tents pitched at ſome diſtance from the barracks, on the ſide of a hill, in a dry and airy ſituation. I narrowly watched the firſt appearance of fever in every one, and directly ſent them to this encampment as ſoon as taken ill.

During the firſt week, and from the 22d to 28th of July, twenty caſes of this fever appeared; and now there was the cleareſt evidence of the fever being contagious. In the ſecond week, or from the 29th of July to 4th of Auguſt, ſixteen caſes appeared; from the 5th to the 11th, only ten caſes appeared; and from the 12th to the 18th, eight.

The fever, in every inſtance, was the *Typhus*, or that uſually denominated Low Nervous Fever. The only peculiarity attending it, was the very ſudden attack in ſeveral caſes with epilepſy, delirium very quickly ſupervening, and the abſence of a cold ſtage in moſt inſtances. The pulſe was at firſt remarkably full and hard, ſo much ſo, as in two inſtances to lead me to open a vein.

Whether a tendency to the bowels was part of the diſeaſe, or might be owing to the early and liberal uſe of the bark, I cannot determine, for I thought the uſe of that medicine too indiſpenſible to lay it aſide for a doubtful experiment.

In no caſe did the fever put on a remittent form, though one man, after being diſmiſſed, and ſent to barracks, has had an intermittent.

In the *treatment*, I found it in every caſe neceſſary to clean the ſtomach and bowels. Immediately on doing which, and during the paroxyſm, I gave James's, or the antimonial powder, in repeated doſes. At the time that I gave the antimonial, I likewiſe exhibited the bark and wine. I generally made the patient take of decoction of bark, a quart in twenty-four hours; and added ſeneka, aromatics, or laxatives, as occaſion indicated.

It may be worth remarking, that the firſt and only caſe that proved fatal, (viz. Skinner), was treated on the antimonial plan; and no bark was exhibited, nor wine in ſufficient quantity.

Bliſters gave great relief where there was headach, and an opiate was generally wanted at bed-time.

I have already ſaid, that in two caſes I

had

had recourſe to the lancet. But my ſucceſs in theſe was not ſuch as to encourage me to try it in others. There was no buffy coat on the blood of either; and the proportion of ſerum in one of them, was greater than I have ever ſeen it in any caſe whatever.

I found it in general neceſſary to continue the bark a conſiderable time after there was a complete remiſſion.

In this fever, there was the greateſt proneneſs to relapſe, which, in ſix or eight caſes, happened no leſs than three times. The moſt trifling irregularities, I found, were ſufficient to occaſion this.

In attempting to aſſign a *cauſe* for this fever, after having given it ſome conſideration, and conſulted the gentlemen who acted with me, I find myſelf ſtill at a loſs. There is certainly no one cauſe adequate to account for its origin; ſeveral muſt be taken into the account; all of which, I think, it may be ſhewn, had their ſhare as cauſes.

1ſt, The ſituation of the barracks at St Owen's, is very far indeed from being favourable to the health of troops. The barracks are ſituated on the weſtern ſide of the iſland of Jerſey, cloſe on the beach of the bay of St Owen's, about 170 yards from high-water mark. The bay extends from north to ſouth, about five Engliſh miles. Behind the barracks, there is an extenſive flat or meadow, of a ſemicircular form; which behind is terminated by high and rocky land. The whole of the meadow is marſhy; and on the right wing of the barracks, at but a few yards diſtance, there is a freſh-water lake with a ſlimy bottom, and ſo ſhallow as to be dry in different parts at mid-day. From behind there is but little wind; what comes is either over the two points which terminate the bay, or in front, when it blows over the large Weſtern Ocean. The prevailing wind, ſince our arrival in the iſland, (as will be mentioned afterwards), has been weſterly. And in the evenings and mornings, if it blow but a little, a perſon near the barracks feels himſelf getting wet, from a miſt which comes from the beach, and which is nothing but the particles of water thrown with violence from the tops of the waves. In the day-time, for a very obvious reaſon, this does not take place.

From ſunſet until the riſing of the ſun again, the meadow behind and on each ſide of the barracks, is generally overſpread with fog, and the evenings are very raw and cold, though from eight o'clock in the morning, till three P. M. the heat is very conſiderable. The moiſture of the air near the barracks, from theſe two cauſes, is ſo great, that metals ruſt nearly as ſoon as they do in the Weſt-India iſlands.

2dly, As a cauſe, may be mentioned *the ſtate of the weather*. The wind, in July and the beginning of Auguſt, was weſt, or ſouth-weſterly. At mid-day, Fahrenheit's thermometer in the ſhade ſtood at 75°. There fell but very little rain. I had not an opportunity of examining the quantity, but it was very ſmall. About the 7th of Auguſt, we had ſome refreſhing ſhowers, with much thunder and lightning. The wind got round to the north and north-eaſt; about ſix days after, it got to the weſt and ſouth-weſt; and the thermometer, which had fallen, got up again. On the 14th or 15th, we had again rain, with north-eaſt winds, and the ſame change in the thermometer.

After this, it got again to ſouth and ſouth-weſt. It is a curious circumſtance, and what ſtruck me, that on looking at a regiſter which I have kept, and examining the days on which men were taken ill, and the numbers taken ill on every day, I have found a wonderful correſpondence. For inſtance, in the laſt week of July, there was generally from four to ſix taken ill daily. On the 7th, 8th, 9th, 10th, 11th, and 12th Auguſt, two, one, and none. On the 13th, two; and 14th, four were taken ill; on the 16th, three; and on the 17th, three.

3dly, To the two laſt, as principal cauſes, may be added the great *intemperance* of the men. In Jerſey, ſpirits, and of a very bad quality, are extremely cheap. It was ſome time before the men found out the different houſes neareſt the barracks where gin was ſold; but when theſe houſes were generally known, the greateſt attention from their officers, and the moſt exemplary puniſhment, could hardly prevent a general ſtate of drunkenneſs. To procure gin, they would part with proviſions, neceſſaries, and every comfort. It has been lately diſcovered, ed, that, in order to procure ſpirits, the men, as they were narrowly watched by their officers in the day-time, (the barracks being open and expoſed on every ſide), uſed to go out to the ſpirit-houſes by day-break, and while the dew was all over the meadows. By looking back, I find that ſeveral of them taken ill, had the firſt attack of the fever immediately after getting up in the morning, though they ſaid they had gone to bed at night in good health. At that time, this appeared unaccountable to me.

4thly, To prepare the regiment for a review by the Commander in Chief, on the 23d of July, the men were pretty often out at exerciſe. This, when added to the other cauſes, may have had ſome ſmall ſhare.

5thly, It may deſerve notice, that the major part of our men were raw recruits; and, by not being ſeaſoned, would be predіſpoſed to the operation of the above cauſes.

6thly, To the other cauſes may be added, the quality of the *water* which the men uſed. It is taken from two places; in one of theſe, the water is often brackiſh, and the other ſeems to contain ſelenite.

That every one of theſe circumſtances mentioned had ſome ſhare as cauſes, the different caſes which are preſerved, ſhew.—There may be other cauſes, which I have not been able to trace; but the two firſt mentioned are undoubtedly the principal.

I conceive it not to be of the ſmalleſt moment, whether we can trace infection directly from one to another of thoſe ſeized with this fever. The fact is indiſputable, that the fever ſoon became contagious; by diligent inquiry, we could in a great many inſtances trace it. Of nine orderly men, employed to take care of the ſick, five have been attacked with it, beſides one nurſe. Of the guard ſent to camp, ſome were daily taken ill; and the fever prevailed moſt in that part of the barracks neareſt to the hoſpital.

The cauſes mentioned exiſting, they would operate more powerfully on a few, where there might be prediſpoſition; and having excited fever in them, it is extremely conformable to medical facts, which we have on the beſt authorities, that from ſeveral of theſe men being crowded together, contagion would be generated. But I conceive it to be infinitely more uſeful to inquire, *when generated, how contagion is got rid of?* As in our efforts we have had conſiderable ſucceſs, I hope that what I have to ſay will not be deemed unworthy of attention.

1ſt, By ſeparating thoſe taken ill, on the firſt attack, from thoſe in health, great benefit was obtained. We had nearly a mile to carry them; and many of them found themſelves better by the time they reached the tents.

By thus taking away from the maſs of the contagion, at the ſame time that the danger was leſſened to thoſe in health, the ſick, by being expoſed to a current of air, and having clean bedding and cloaths as ſoon as they reached the tents, were free from petechiæ, and all the bad ſymptoms which appeared in Skinner, and all the firſt caſes which were kept a few days in the hoſpital or barracks.

2dly, Soon after the appearance of the contagion, a part of the regiment was encamped;

camped; and theſe, added to about eighty men, whom I had in tents or the hoſpital, and the men on duty, made together about 200, or half the regiment. Thinning the barracks ſo much was, without doubt, of the greateſt ſervice.

3dly, I aſcribe a great deal to the conſtant and unwearied attention paid to the cleanlineſs and order of the men, when the alarm was ſounded, of our old enemy, *the putrid fever*, being again amongſt us.

4thly, The doors and windows of every room in the hoſpital and barracks were conſtantly kept open every dry day. The whole of the bedding was kept out during the middle of the day, warm vinegar was frequently ſprinkled about every room, and all the walls were white-waſhed.

5thly, What I think has been of the greateſt ſervice in purifying the air of the different rooms, and in expelling contagion, is the extrication of the nitrous vapour, after Dr Carmichael Smith's manner. This my two aſſiſtants, Mr Bruce and Mr Brown, did in the different rooms, both of the hoſpital and barracks, daily. The rooms by this proceſs were rendered ſweet, and the men themſelves ſoon became ſenſible of the comfort of it. We attempted likewiſe the extrication of muriatic acid gas, but with great inconvenience; and it is obviouſly not ſo proper as the nitrous.

6thly, Not a little has been effected by overcoming the general depreſſion of ſpirits and dejection which prevailed, when it was known how ſuddenly and in what ſtate the the firſt caſe died, and when it was ſeen how many were ſeized with the fever at one time.

At firſt, it was with the greateſt reluctance that thoſe taken ill could be made to leave the barracks, and go to the tents. But, when they ſaw that, after going there, no body died, they had more confidence, and went willingly to the tents as ſoon as taken ill.

7thly, The bedding, as well as every article belonging to a man with fever, was carefully ſent with him always. And I ſhall take the precaution to have every thing belonging to thoſe that recover well waſhed, aired, and fumigated, before they return to the barracks. Theſe are the means that have kept this fever, truly alarming at its firſt appearance, ſo moderate in its ſymptoms afterwards.

V.

Account of a Caſe in which a Fiſtula in Perinæo was ſucceſsfully healed by the introduction of a Seton. By Mr Adam Burt, *in the ſervice of the Honourable Eaſt-India Company. Communicated in a letter to Dr* Charles Alexander Graham, *Phyſician, Stirling.*

I Had the good fortune, not long ago, to accompliſh a cure, with very little trouble, in a very untoward caſe. Mr —— of the Bengal civil ſervice, had a fiſtula in the perinæum of conſiderable ſtanding. The external opening was by the ſide of the anus, or at leaſt very nearly as low. The urine was voided in conſiderable quantities by the ſinus.

I propoſed that the ſinus ſhould be laid completely open up to the urethra, which is the mode of cure recommended by every modern ſyſtematic writer. But the patient would not ſubmit to the operation.

I was, accordingly, compelled to try ſome other expedient; and naturally thought of a ſeton; which, it is ſurpriſing, is not ſuggeſted in this diſorder, as far as I remember, by any writer in ſurgery.

I introduced a directory at the opening, and carried it up to the junction of the ſinus with the urethra. This was a painful operation; for the ſinus *burrowed*, if I may uſe the expreſſion, and the ſkin was thick and hard all over the diſeaſed parts.

I punctured down by the ſide of the urethra, (carefully avoiding it), to the directory, which then received the end of the probe-needle, armed with the cord.

By thus introducing the ſeton from above, the cord in dreſſing was drawn downwards, and ſo favoured the diſcharge. In ſix or eight days, the diſcharge became of a healthy appearance and conſiſtence. A conſiderable inflammation was excited all around. Ever after the introduction of the ſeton, the urine flowed the natural way.

I withdrew the ſeton on the 9th day from the operation, and applied a compreſs, wet with camphorated ſaturnine ſolution, ſecured by a bandage, ſo as to give as much preſſure as could be well born by the patient, but avoiding the urethra. I uſed no bougies, fearful of irritation. The parts healed by the firſt intenſion.

Few caſes are more diſtreſſing than perinæal fiſtulas. Surely a ſeton* ought always to be tried; for ſhould it fail, the ſinus may afterwards be opened.

VI.

The Hiſtory of a Caſe terminating fatally, from a Concretion formed in the Bowels, in conſequence of ſwallowing the ſtones of Fruit. Communicated to Dr Duncan, *in a Letter from Dr* Charles Marſhall Clarke, *Phyſician, Lowth.*

SEEING a ſingular caſe in your Commentaries, reſpecting the effects of ſwallowing the ſtones of fruit, permit me to ſend you the following.

Mr Withington, a tradeſman, forty-eight years of age, of a ſtrong habit of body, was ſuddenly taken ill of a violent pain in his bowels in the night-time. His complaints continued moſt exquiſite for two days before I ſaw him; during which time, he had ſeveral clyſters injected without any paſſage, they having returned juſt as thrown up. Fomentations

tations he had alſo uſed, but without ſucceſs.

At my coming, I found him complain of moſt acute pain on the right-ſide of the hypogaſtric region, and, as it appeared both to him and me, in one point. I examined the part with my hand, but found, neither there nor in any other part of the abdomen, any preternatural ſwelling or hardneſs. His pulſe was ſlow and regular, ſuch as is uſual in health, without the leaſt appearance of fever, except thirſt ſometimes. He ejected every thing that he ſwallowed except chicken-broth; and even this he kept only now and then. This vomiting began with his pain, and continued to the laſt, at intervals, unleſs when he had opiate clyſters, which always ſtayed it, and gave him immediate eaſe and reſt. On the contrary, thoſe clyſters that operated with any degree of ſtimulus, increaſed his pain exceedingly.

Though I made uſe of every mean to prevent the inflammation before it came on, as alſo to check it after its appearance, yet all proved ineffectual. For, on the 4th day in the evening, I perceived a quickneſs and hardneſs in his pulſe, which was ſucceeded by much heat and thirſt; after which he ſweated violently all night. Theſe ſweats, towards morning, became clammy; and before noon, a coldneſs in the fingers and toes ſeized him, which gradually advanced to his hands, arms, feet, and legs. The pulſe at the wriſt grew weaker and weaker, until it became quite imperceptible; above the elbow it was ſtill ſufficiently ſtrong; but as the coldneſs advanced, it gradually died away, whilſt the temporal arteries ſeemed not to loſe any thing of their uſual vigour for ſome time. Thus he gradually declined till the evening; when, about eleven o'clock at night, he died.

From the violent vomitings he was ſeized with through his illneſs, an inverſion of the periſtaltic motion of the bowels might naturally have been expected; yet he never ejected any thing fœtid, or of an ill ſcent, nor indeed any thing more than what he took in, together with the bile and humours of the ſtomach. From his vomiting, his pain in the right hypogaſtric region, his complaining

ing ſometimes of pain in his loins, his formerly having had ſymptoms of the gravel in his kidneys; from his large inteſtines being cleared by clyſters, even to a diſcharge of the thin excrement of the ilium; and from his whole abdomen being ſoft and eaſy; I was led to conjecture, for a while, that he had a ſtone lodged in the right ureter. But, on conſidering the caſe more maturely, for the following reaſons, I rejected my firſt opinion. His urine was natural, and in proper quantities, until the inflammation came on, when it became of a flame-colour, with a little oilineſs upon its ſurface. His pain (in his own manner of deſcribing it) was more forward, or between his navel and right groin, and ſuperficial. Another more convincing reaſon, that his complaint was actually in his inteſtines, and did not ariſe from a ſtone in the ureter, was his having ſwallowed a conſiderable number of the ſtones of Damaſcenes; tarts of which he had eat the laſt dinner before his illneſs; from which time he never had had a ſtool, ſo that all thoſe ſtones were lodged in ſome part of the inteſtines; which made me ſuſpect, that they might be the cauſe, by being connected together by the mucus, hardened fœces, &c. and blocking up the paſſage through ſome of the ſmall inteſtines. This conjecture obtained more upon me by having brought away two or three of thoſe ſtones by clyſters, in a ſmall portion of excrement; and in this opinion I continued till his death.

In all my conjectures, reſpecting the ſituation of the obſtructed inteſtine, I was miſtaken. For, upon opening him, we found, on examining the inteſtines, a hard lump of ſomething in the ilium, which ſeemed then to be moveable in ſome degree; but, above it, the gut was full of liquid fœces, in which flowed a number of the Damaſcene ſtones, and appeared gangrenous and mortified, for about three or four inches above this hard ſubſtance, with a ſmall breach in the mortified part, through which iſſued out the fœcal matter of the ilium. Having tied this ſubſtance above and below, we examined again the kidneys, ureters, bladder, gall-bladder, liver, pancreas, &c. as alſo the whole of the inteſtines; but found every other part in a ſound and natural ſtate, except where this hard ſubſtance was. We took it out, and on examination found it to be a hard ſtony concretion, with protuberances ſomething like the Rad. ariſtolochiæ rotundæ, or a moderate-ſized potatoe, weighing one ounce and fourteen grains, of the colour of Bezoar.

This perſon had a very good conſtitution, not liable to complaint, except, for ſeveral years paſt, a perpetual coſtiveneſs, which generally obliged him to carry ſomething about with him to keep his body open. This concretion had three of the above-mentioned protuberances broken off, two were filling up again, and the third ſeemed very freſh, as if broken off lately.

The obſtruction was a little below the beginning of the ilium, where, I doubt not, it ſtuck faſt during the patient's lifetime, though it appeared to be ſomewhat looſe when dead, from the then relaxed ſtate of of the inteſtines.

Upon dividing the ſtone with a ſaw, we found a large plum-ſtone, with its kernel perfect in it, which, no doubt, was its nucleus, and the primary cauſe of its formation.

Whence we may obſerve, how neceſſary it is to avoid the ſwallowing of fruit-ſtones, contrary to that vulgar error, that they are good for digeſtion, or that the fruit will never hurt, provided you ſwallow the ſtones. This was the notion of this unhappy man, who from his infancy had conſtantly ſwallowed the ſtones of fruit he ate.

Bonetus, in his Sepulchretum Anatomicum, Riverius, in his Obſervations, and others, make mention of ſtony concretions being found in the ſtomach; yet none I know of take notice of the cauſes producing them; nor do I find any examinations to that purpoſe. Might not thoſe caſes ariſe from a ſimilar cauſe with the above? and, might not this ſtone have had its firſt formation in the ſtomach?

VII.

Account of Fourteen Men of the Royal Artillery at Quebec, who were nearly poiſoned by drinking a decoction of certain Plants. By George Longmore, *M. D. Communicated to* Dr Rollo, *Surgeon-general to the Royal Artillery at Woolwich.*

Quebec, May 27. 1798.

NOVEMBER 20th, ten o'clock, A. M. 1797, I was ſent for in great haſte to viſit ſeveral men, who were reported to be poiſoned at the Artillery Barracks.

Being on my way to the hoſpital, which is within the ſquare, I was at the place without delay.

On entering the barrack-room in which the men were lodged, I was much alarmed at the ſcene of diſtreſs which preſented itſelf to me on every ſide. I inquired what had produced ſuch violent effects, and was informed, that all the men of the meſs, (off duty), fourteen in number, (viz. John Walker, aged 36; David Powell, aged 30; William Mills, aged 31; Richard Hodgin, aged 37; John Walker, aged 36; Eley Riley, aged 38;—of Major Hamilton's company; Thomas Henderſon, aged 37; William Coffer, aged 28; William Douglas, aged 39; Robert Hillhouſe, aged 41; William M'Currach, aged 36; William Lethbridge, aged 36; John Grant, aged 28; and William Maddox, aged 33;—of Major Glaſgow's company), had drank for breakfaſt a ſtrong decoction of herbs, which one of them (John Walker) had gathered in the neighbourhood of this garriſon, as an Indian tea, in very general uſe among the lower claſs of inhabitants in Canada. Soon after drinking it, *i. e.* in about from twenty minutes to an hour, moſt of them were taken ill, and ſeveral now appeared to me in the moſt imminent danger.

The ſymptoms with which they were univerſally attacked, though leſs or more violent in degree, were firſt, as they expreſſed it, a lightneſs in the head, vertigo, a confuſion of objects before their eyes, an inability to walk, and a ſickneſs at the ſtomach, which ſoon increaſed to violent retching and vomiting. In ſome caſes, the patient was much diſtreſſed, after the ſickneſs came on, before he could diſcharge the contents of the ſtomach. The pulſe, in all, was ſmall, ſlow, and irregular.

In the worſt caſes, particularly in that of Lethbridge, the ſymptoms already mentioned were followed by cold ſweats, and cold extremities. The countenance was pale, the features ſhrunk, the eyes fixed and glaſſy, with an intermitting pulſe, at times ſcarcely to be felt. The reſpiration highly laborious, with low muttering and coma.

I was ſo much alarmed at ſeeing a number of men in this new (I may call it) and critical ſituation, that I immediately diſpatched the orderly man of the hoſpital, to requeſt the aſſiſtance of my friend Dr Nooth, who, in every caſe of emergency, and indeed upon all occaſions, is ever ready to ſhew his humanity, and to exert his profeſſional talents, for the good of his Majeſty's ſervice.

In the mean time, as I had obſerved that violent vomiting had come on, in almoſt every inſtance, and that it appeared to give at leaſt a temporary relief, I thought it adviſeable, in order to dilute and expel the poiſonous decoction from the ſtomach, to promote this indication of nature. I therefore ordered plentiful draughts of hot water to be given to each perſon, and to be particularly puſhed in thoſe caſes where the ſymptoms appeared the moſt urgent, till a copious vomiting was excited. In ſeveral inſtances this plan appeared to give conſiderable relief, and the men ſaid they felt much better. But in others, and particularly in the caſes of Lethbridge and Hillhouſe, the deleterious effects of the poiſon continued extremely alarming. The former had all the urgent ſymptoms already enumerated, which threatened immediate death. As I durſt not, under ſuch circumſtances, venture to puſh the plan of exciting vomiting further, (and I really found the excitability of the ſyſtem ſo far ſuſpended, that it was in a great degree impoſſible), I inſtantly reſolved on having warm ſtimulant clyſters thrown up, which, by being repeated, produced at length a copious evacuation. And this, he afterwards told me, was the firſt impreſſion he was ſenſible of. He, however, again relapſed, and was (though in a leſſer degree) in a comatoſe ſtate when Dr Nooth arrived.

The Doctor ſuggeſted the uſe of cordials; and hot wine and water, the one moſt readily come at, was inſtantly had recourſe to. I mixed port-wine with boiling water, nearly in equal portions. But Lethbridge was now in ſuch a ſtate of inſenſibility, that I made ſeveral attempts before I could get any down. Rouſing him, by ſhaking and calling loud to him, we at laſt ſucceeded in getting him to ſwallow ſeveral table-ſpoonfuls. He ſoon became more ſenſible of what was paſſing round him, and I now gave him ſeveral ounces of the hot wine and water. Upon this, his face, which before was pale, dejected, and the features contracted, appeared more natural, and he became fluſhed, which ſhewed a returning circulation. His pulſe, however, continued low, irregular, and intermitting. He was again, in a ſhort time, ſeized with a ſevere fit of vomiting, which appeared effectually to clear his ſtomach. As ſoon as I thought he could retain it, I gave him a few ounces of hot wine and water. This remained on his ſtomach, and he gradually became more rational, and began to recover.

At this time, Hillhouſe, although he vomited freely on being firſt affected by the poiſon, and did not appear more diſordered than ſeveral of the other men, was brought into the barrack-room in a ſtate of ſyncope. On recovering a little, he got upon his feet, ſupported by two men. In this attitude, with his eyes fixed, and ſtaring, he was convulſed; and, as if dancing for a minute or two, he ſprang from the floor, ſhewing geſticulations equally odd and fantaſtical as thoſe attending the Chorea St Viti. I got him put into his birth as ſoon as I could, and gave him ſeveral ſpoonfuls of hot wine and water, which appeared to relieve him; for he ſoon became ſenſible, and complained of nothing but an oppreſſion of breathing, which gradually went off, leaving only a general debility.

I had now time to pay more attention to

the leſs urgent caſes. On the principle, that it would be of the greateſt conſequence to have the men's bowels, who had been moſt materially affected, freely opened, I made a ſolution of the natron vitriolatum, to which I added ſome of the tinct. lavendulæ compoſita, to make it ſit better on the ſtomach; and gave a ſufficient doſe to ſuch of my patients as I thought likely to retain it. I likewiſe directed oat-meal gruel to be made; of which, in an hour, they were deſired to drink freely. And ſoup, made with freſh beef, was ordered to be prepared, without being made too thick with vegetables, for thoſe who ſhould wiſh to have ſome for dinner.

Having now the pleaſure of ſeing my patients in a fair way of recovery, I went to the hoſpital, where I had not been many minutes, before I was informed, that Coffer (the only man in the fourteen, who, though he had drank of the tea, had, till now, remained altogether unaffected by it) was ſeized in the ſame way as the others. This was four hours after ſwallowing the poiſon; and during the time that the others had been ſo ſeverely attacked, this man, though he had drank as much as any of the tea; boaſted of feeling nothing of it, and ſeemed certain that he would eſcape its deleterious effects.—This man is ſubject to epileptic fits; and I wiſh particularly to mention this peculiarity in his caſe, to ſhew, that it does not appear, that his attacks of epilepſy proceed from inordinate irritation acting on the nervous ſyſtem; but that, on the contrary, there would ſeem to be a more than uſual want of ſenſibility, at leaſt in the nerves of the ſtomach. For, while the other men (as obſerved above) were under ſuch alarming circumſtances, he ſeemed to walk about with an air of triumph and unconcern.

I returned to the barrack-room immediately, and found that he had been attacked with a very ſevere fit of vomiting; but that he had ſuffered very little from the ſymptoms of lightneſs of the head, dizzineſs, vertigo, &c. ſo diſtreſſing to the others, previous to his diſcharging the contents of his ſtomach.

I now gave him ſome hot wine and water, as in the other caſes. This remained on his ſtomach, which had before been completely emptied, and he ſoon recovered from the violent ſymptoms attending the action of the poiſon. But, as in my other patients, his pulſe continued low, and there appeared a general languor and debility in the ſyſtem, during the whole of the day.

I again viſited my patients at half paſt two o'clock, and found that they all continued to recover. Severals of them told me that they had now no complaint, and that their appetites had returned. They had been able to take either gruel or weak ſoup, which had remained on their ſtomachs. Notwithſtanding theſe favourable circumſtances, there appeared great languor and debility in all the men. Their pulſes were below the natural ſtandard, and in ſome inſtances very irregular.

I muſt here obſerve, that I was much ſurpriſed to find, that in no inſtance, not even in Lethbridge's caſe, where the collapſe was ſo great, did the re-action of the vaſcular ſyſtem take place to ſuch a degree as to require the uſe of the lancet. Nor was the thirſt much increaſed, nor febrile heat on the ſkin obſervable, except in one or two inſtances.

In the evening, when I viſited my patients at ſeven o'clock, I found them all better. But there ſtill remained a general debility and want of energy in the ſyſtem. And this appeared to extend to the alimentary canal; for, though the natron vitriolatum, which I had given in the forenoon, had not, in ſeveral inſtances, been rejected, it had produced the deſired effect only in one or two patients, who were completely relieved.

It appeared to me, on this occaſion, highly neceſſary (as it does in the firſt ſtage of all diſeaſes) effectually to open the bowels. I therefore prepared a purging draught for each of thoſe who had not before had their bowels opened, which I deſired to be given early in the morning.

21ſt. I viſited the barrack-room at ten o'clock. I found that the men had all ſlept well; and that thoſe who had taken the purging draught early in the morning, had had their bowels effectually opened. They had now no complaint, but a ſlight degree of weakneſs, and a kind of awkwardneſs about the head, which went off in the courſe of the day. And, on the 22d, all were returned for duty, except Hillhouſe, who, for a year paſt, had been in the ſick-report, with ſymptoms of incipient phthiſis pulmonalis, and general debility.

As ſoon as the urgency of the caſes given in the above detail would permit, we began to inquire more particularly into the nature of the tea, which had produced ſuch alarming and deleterious effects. On examining the plants which compoſed the decoction, (for I found, on inquiry, that they had boiled them), we found three different genera of the claſs of Decandria, viz. the *Ledum*, the *Andromeda*, and the *Gualteria*. Theſe plants were collected from the ſame ſoil and ſpot, in the neighbourhood of this garriſon.

The Ledum paluſtre is very commonly uſed as a tea by the lower claſs of inhabitants in this country, under the name of Labradore Tea. The Andromeda is a rough and diſagreeable bitter. But the Gualteria is more pleaſant and grateful. A tea of this laſt is frequently uſed by the Canadians, and is ſaid to be a cooling and grateful ptiſan in fevers.

Many of the plants of this claſs appear more or leſs poiſonous. They are extremely active when taken in quantity, and may perhaps, at ſome future period, be introduced into practice as remedies of conſiderable efficacy.

A gentleman, now of Quebec, who was many years reſident among the Indian tribes inhabiting the north ſhore of the river St Laurence, towards Hudſon's Bay, informs me, that theſe ſavages, when they cannot come at ſpirituous liquors, employ a ſtrong decoction of theſe plants, particularly of the Andromeda, to intoxicate and ſtupify themſelves, theſe terms being ſynonymous with a ſavage; that this decoction ſeems to exhilirate them, for a little time, almoſt to a a ſtate of frenzy; but that, after this, they appear to fall into a ſtate of almoſt total inſenſibility, which laſts for a day or two.

The decoction taken by all, was prepared at the ſame time, and in the ſame tin-boiler. It was, of courſe, of an equal degree of ſtrength, and the allowance to each man was the ſame, viz. a quart-bowlful, *i. e.* the quantity of the tea they are by regulation allowed at their breakfaſt meſs.

Corporal Walker, of Major Hamilton's company, finding the decoction too ſtrong and diſagreeable, had diluted it by the addition of water; and by that means nearly eſcaped its poiſonous effects. He afterwards told me, that he believed it was more from the impreſſion made on his mind, by ſeeing ſuch a ſcene of diſtreſs around him, and from his dread of conſequences, which were ſtill doubtful, that he was rendered ſick, than from the quantity of tea which he had drank.

Walker, the private, who gathered the plants, was violently ſick at ſtomach, with ſevere vomiting, and the whole ſyſtem was very much affected. But the ſymptoms were much leſs alarming than in Lethbridge and Hillhouſe's caſe. Of the former I have to obſerve, that in the month of July laſt his life had been deſpaired of, in conſequence of a high degree of inflammation, and ſeeming extravaſation, brought on in the cheſt by over exertion in raiſing a twenty-four pounder. By repeated blood-letting, with a ſtrict attention to the antiphlogiſtic regimen, the diſeaſe was removed; but he remained for ſeveral weeks in a ſtate of extreme debility. He had, however, been doing his duty for at leaſt two months previous to this period.

Lethbridge was naturally the ſtrongeſt man in the detachment of Royal Artillery in Canada. He could at one time raiſe 500 ℔. with one hand. His conſtitution muſt, however, have received ſuch a ſhock by the former diſeaſe, that a particular irritability and debility muſt have remained, which rendered the poiſon more active in his caſe than any other, and made his recovery, at one time, extremely doubtful.

In Hillhouſe's caſe, I have further to obſerve, that after the narcotic effects of the poiſon had acted with great violence on the ſyſtem, the *fantaſtic*, if they may be ſo termed, ſupervened; for he, as already mentioned, had gone into the open air, as a convaleſcent, from the firſt attack, where the head and ſtomach had been only affected, when he fell down in a fit. He was brought into the room in this ſtate; and, recovering a little, he exhibited the fantaſtic geſticulations deſcribed above. This man was in a ſtate of general debility from previous diſeaſe. He had been diſcharged a few weeks before from the hoſpital, after being many months on the ſick-report, with ſymptoms of incipient phthiſis.

VIII.

The Hiſtory of the Caſe of a Fœtus found in the Right Ovarium. By Dr Forreſtier, *Chevalier of the Order of the King of France, Phyſician to his Army in North America, and Firſt Phyſician to his Royal Highneſs Monſieur,*

I Was called on the 18th of March 1784, to ſee the wife of a porter who had been troubled with colic pains from the month of November preceding. I found her conſiderably emaciated, with a feveriſh, ſmall, and hard pulſe, frequent retchings without effect, and a continual teneſmus. She told me ſhe had not menſtruated ſince the month of September laſt; that at that period ſhe fell much in the ſame way ſhe had done formerly in four preceding pregnancies, which had all terminated happily; that for the two firſt months of this, ſhe had enjoyed perfect health, but upon a journey to Fontaine-Bleau, where ſhe had buſineſs, ſhe was ſeized with violent colic pains in her lower belly, which had continued ever ſince; and that at that period her pains were conſiderably increaſed.

After this account, having no doubt of the patient being in a pregnant ſtate, I ordered a man-midwife to be called, who confirmed my diagnoſtic; and we preſcribed a proper treatment, in conſequence of which the pains were quieted during three weeks.

On the 8th of April following, the pains returned, with a violent ſhivering, a great degree of fever, and a conſiderable loſs of blood, very black, and mixed with hard clots of a fleſhy appearance.

Upon examining the uterus, its orifice did not appear to be dilated. I ordered a bleeding at the arm, and afterwards a ſecond, after which ſhe paſſed a tolerable quiet night. As the circumſtances of the patient left us in great uncertainty what to conclude, we continued to queſtion her very particularly, when, to our great aſtoniſhment, ſhe told us ſhe believed ſhe paſſed theſe clots by the anus. We immediately ordered a glyſter to be adminiſtered, which was ſoon followed by a ſtool of black blood, in which there floated two clots of the ſame nature with the former ones, although during the diſcharge the precaution was taken to ſhut the paſſage from the vagina.

No longer doubting of the fact, the next queſtion was, to diſcover from what veſſels the blood came. The matter diſcharged had not the appearance of the morbus niger, and had ſtill leſs reſemblance to the hæmorrhoidal flux. Could the phœnomena be accounted for, by ſuppoſing an adheſion of the rectum to the uterus?

In this ſtate of doubt reſpecting the true nature of the diſeaſe, it was ſtill impoſſible not to ſee the danger attending it. The continuance of the hæmorrhage, and the fainting fits with which it was followed, terminated in the death of our patient, on the 10th of April, about eight o'clock in the evening.

We proceeded next day to examine the body, by diſſection, aſſiſted by Mr Gauchez ſurgeon and man-midwife, and Mr Villète apothecary. We found the liver, ſpleen, ſtomach, and ſmall inteſtines, in the ordinary ſound ſtate. But both the right and left iliac regions contained each a tumour. The uterus lodged in the left was of the ſize of a new-born child's head. It was of a whitiſh colour, all the blood-veſſels appearing obliterated. Having opened it through its whole extent, we found its coats about an inch thick at the fundus, and gradually diminiſhing to its cervix. This organ, though a little dilated, approached a good deal to its natural ſtate, and the cavity would hardly have contained a ſmall hen's egg.

We then proceeded to examine the tumour which occupied the right ſide. It had the form, and was nearly the ſize of a large hog's bladder diſtended with air. It had a general adheſion to the neighbouring parts, which began from its bottom, to the inteſtinum cæcum, and embraced the whole of the colon to its ſigmoid flexure, where it appeared engrafted in the tumour. It was alſo attached to the meſenteric part of the large inteſtines. The epiploon was ſlightly united to it, and as it were laid upon its ſuperior and anterior part. In ſhort, it ſeemed to have attached itſelf intimately to all the neighbouring viſcera. Theſe had preſerved their natural colour, except the inteſtines, which, from the cæcum to the rectum, were black, and filled with blood.

After having eaſily ſeparated the adheſions of the tumour, except at the point correſponding to the ſigmoid flexure of the colon, where a very firm adheſion was formed, we proceeded to examine the Fallopian tubes. The left was in its natural ſtate. Its opening into the uterus was free, though a little more difficult than in the ſound condition of that organ. We then paſſed to the right tube, the canal of which was obliterated, and reſiſted the introduction of the ſmalleſt probe. The inſtrument ſeemed likely to penetrate into the tumour, but the reſiſtance it met with was too ſtrong not to make us afraid of occaſioning a forced paſſage, which would have thrown uncertainty upon the reſult of our reſearches.

Having made this examination, and being convinced that this tumour was nothing but the right ovarium prodigiouſly dilated, we opened it through its whole length. We found it filled with blood, and with clots reſembling theſe which the patient had voided by the anus.

Having directed the hand to the point of adheſion of the inteſtine with the tumour, we drew from it a very conſiderable clot, which had formed itſelf there. This confirmed us in the opinion of the exiſtence of a communication between the tumour and the inteſtine. Proſecuting our reſearches farther with regard to the contents of the tumour, we felt bones which we diſcovered to be the entire ſkeleton of a fœtus almoſt totally putrid. The membranes, however, of the umbilical cord were not entirely in a ſtate of putrefaction. By means of theſe, we arrived at the placenta, which was placed to the right upon the bottom of the tumour.

The extreme fœtor of this new matrix, and of the inteſtines, would not permit us to continue our reſearches any longer. But the formation and exiſtence of the fœtus in the right ovarium were inconteſtibly proved; and this was the eſſential object.

The new matrix, with the fœtus, removed from the body, were made to undergo ſeveral waſhings with camphorated vinegar, being previouſly put into a baſket, to prevent the disjoining of the parts, the extremities and trunk of which were held together only by their ligaments.

The fœtus was almoſt in a complete ſtate of maceration. All the muſcles, except a very ſmall part of thoſe which belong to the linea alba and the tendinous extremities, were totally deſtroyed, as were likewiſe, the lixer, the ſpleen, and the lungs. There remained nothing of the heart but its coverings. The membranes of the brain were fallen into the cavity of the cranium. There was no veſtige of the parts of generation. The nails of the fingers, however, were preſerved.

The ſkeleton was fifteen inches long, the upper extremities were five inches and a half, and the lower five inches in length. The oſſification was as perfect as that of any infant in the womb. The cotula, the ſternum, the iliac bones, and even the parts which generally are laſt formed, were all perfectly complete.

IX.

Obſervations on the Pemphigus major of Sauvage, with a brief Account of two Caſes of that Diſeaſe. By Dr R. Hall, *Phyſician, Jedburgh.*

THAT Pemphigus is either a diſeaſe of rare occurrence, or that it is only of late the attention of medical practitioners has been excited towards it, is generally admitted. But whether it be contagious or not, ſeems, as yet, not ſo fully aſcertained. I truſt, therefore, that the communication of ſome circumſtances reſpecting it, as they may tend ſomewhat to elucidate the nature of the affection, will not be deemed wholly unacceptable.

The ſymptoms of this affection are now ſo well known, and in the two caſes which fell under my own obſervation, proved ſo analogous to the deſcription given of that diſeaſe

eaſe by Dr Stephen Dickſon of Dublin *, that I ſhould deem it wholly ſuperfluous to enter here into any particular or minute detail of them.

In the months of July and Auguſt laſt, two patients, both adults, were affected with Pemphigus; one in my own family, and the other in a different quarter of the town.

Mrs H—— had, previous to the attack, incurred much diſtreſs from an obſtinate ophthalmia, and been conſiderably reduced by the local and general evacuations employed in combating that affection. M—— L—— had alſo been ſubjected to cauſes inducing debility. Both the patients attributed the complaint to cold; Mrs H—— to a draught of cold butter-milk, taken when heated; M—— L—— to cold externally applied.

The complaint, in both patients, was uſhered in and preceded by laſſitude, nauſea, and headach. To theſe ſymptoms ſoon ſucceeded reſtleſſneſs, a ſenſe of oppreſſion about the præcordia, thirſt, great frequency and irregularity of pulſe, ſoreneſs of the throat,

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* London Medical Journal, vol. IX.

throat, with ſome difficulty of deglutition, parched and foul tongue, and a præternatural heat of the ſkin.

Having never witneſſed a caſe of Pemphigus, it was unqueſtionably owing to that principle of aſſociation, termed the relation of reſemblance, that I was led, upon the appearance of the veſicles, to recogniſe the true nature of the affection, from the recollection of a delineation which I had formerly ſeen, prefixed to a diſſertation by Mr Chriſtie [*], on that ſubject.

Upon a careful re-inſpection of the plate [†], which accurately repreſents the appearance of the veſicles, at the three different ſtages of their origin, acmè, and decay; and, after comparing his and other accounts with the preſent exanthemata, I heſitated not to conclude, that the diſeaſe under which my patients laboured, was the Pemphigus major, the firſt ſpecies of the genus Pemphigus of Monſieur de Sauvages.

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[*] London Medical Journal, vol. X.

[†] Although aware that coloured engravings are, on ſeveral grounds, liable to objections, I am nevertheleſs diſpoſed to coincide in ſentiment with Mr Chriſtie, that the addition of plates may, in a variety of caſes, (but ſtill more particularly, perhaps, in cutaneous affections), be conſidered as an uſeful auxiliary to verbal deſcription. They certainly not only facilitate the juſt conception of the object deſcribed, but fix the idea of the diſeaſe and thus enable future obſervers more readily to collect facts relative thereto.

In both patients, the fever aſſumed a remittent type, but in neither were the exacerbations or remiſſions diſtinctly marked.

No ſimilar complaint occurred at that time, either in the town or neighbourhood.

In Mrs H——'s caſe, new veſicles continued ſucceſſively to appear until the 16th, and in that of M—— L—— until the 10th day, from the time of their firſt eruption. In the former, the phlyctænæ amounted to 28; in the latter, to not more than half that number. Their ſize, in general, conſiderably exceeded that of an almond; and in neither of the caſes did any of them ſubſide, ſooner than the 6th, 7th, or even the 8th day, after their appearance.

The ſerous fluid, with which theſe cuticular veſicles were filled, was in general abſorbed into the ſyſtem, unleſs when diſcharged by a rupture from accident or exertion, in which caſe, it was quickly replaced by a freſh collection.

The veſicles, in the caſe of M—— L——, were wholly confined to the external ſurface of the body, whilſt in that of Mrs H——, much ſuffering and diſtreſs were induced by their preſence on the tongue and fauces. The pharynx was, beſides, inflamed. But there appeared no evidence, from which it could be inferred, that any part of the alimentary canal was affected with them.

Each had, during the continuance of the diſeaſe, a frequent diſpoſition to Leipothymia. But in the caſe of Mrs H——, the intenſity and recurrence of this, as well as of every other ſymptom, were much more formidable than in that of the other patient.

In the caſes now under conſideration, the prevalence of great aſthenia, and an evident tendency of the fluids to putreſcency, ſufficiently indicated the propriety of an invigorating and antiſeptic plan of treatment; by the proſecution of which both patients ſtruggled through the diſeaſe; but Mrs

H——

H—— with much greater difficulty than the other, owing as much to the ſuperior malignity of the affection, as to a relapſe ſhe ſuſtained from an incautious expoſure of herſelf to cold when in a ſtate of convaleſcence.

Whether we are yet in poſſeſſion of a ſufficient number of well-authenticated caſes of Pemphigus, to poſitively warrant us in the induction, that noſologiſts have hitherto erred, in claſſing it in the order of contagious exanthemata; or whether the two caſes, briefly ſtated above, tend to the confirmation of that opinion, are queſtions certainly not altogether unworthy the attention of every philoſophic cultivator of medical ſcience.

The reaſons which favour the opinion, that it is a diſeaſe of a purely ſporadic nature, are, in general, that almoſt all the well-atteſted inſtances of Pemphigus are ſolitary ones. And, reſpecting the preſent caſes in particular, becauſe,

1ſt, Although each of the patients ſlept with others during the whole period of their illneſs, yet none but themſelves were affected with it.

2dly, Although both patients were affected with the diſeaſe nearly at the ſame time, yet they lived at a conſiderable diſtance from each other, and there had been no previous communication between the two families.

3dly, Upon the moſt diligent inquiry, I could not learn that any other perſon, either in the town or vicinity, had laboured under it. Nor have I been able ſince to diſcover, that any perſon whatever has become affected with it.

By the adherents to the opinion, that Pemphigus is in its nature contagious, it may perhaps be contended, that here we have the occurrence of two inſtances of the diſeaſe at the ſame time and place. And ſince, in the moſt epidemic contagions, only a certain portion of thoſe who are expoſed to its influence are attacked, it does not follow, that although, in the preſent inſtance, the diſeaſe ſpread no wider, it was not contagious; for, where no prediſpoſition exiſts in thoſe who intercommunicate with the ſick, the exciting cauſe muſt ever fail to produce the diſeaſe.

It has been already obſerved, that, previouſly to the acceſſion of the complaint, no intercourſe whatever, either directly or indirectly, had taken place betwixt the two patients. Theſe caſes can, therefore, only be regarded as inſulated ones. And, examining the queſtion regarding the abſence of prediſpoſition *a priori*, it muſt ſurely appear much more probable, from the various degrees of ſuſceptibility of different conſtitutions, that ſome would have become ſubjected to the action of the contagion, had it actually exiſted, than that the contagion ſhould have remained inactive, through want of the neceſſary aptitude or prediſpoſition.

That prediſpoſition muſt concur, to render contagion ſufficiently active to produce diſeaſe, is a propoſition, the truth of which is not meant to be diſputed. I would only contend, that any argument derived from the ſuppoſed abſence of prediſpoſition, will not apply, in the preſent inſtances, to explain the non-production of the diſeaſe. Since, of the many individuals who, impelled by different motives, viſited the ſick, beſides the uſual attendants, who, by their conſtant intercourſe with them, muſt have been more peculiarly expoſed to the action of the contagion, not one was attacked, although ſome of them were evidently in ſuch a ſtate of both body and mind, as muſt have rendered them liable to be acted upon by the occaſional cauſe, had it been applied.

Such a complete exemption, then, cannot fail to enforce the rejection of any argument adduced from the preſent caſes, in ſupport of the opinion, that the diſeaſe in queſtion is really contagious. That it varies much as to its character of mildneſs and malignity, cannot, I conceive, be called in queſtion, if the teſtimonies of practitioners are at all to be relied on. And its complication with other diſeaſes may perhaps ſatisfactorily explain that diſcordancy of ſentiment which prevails amongſt different writers concerning it.

In every caſe where the degrees of evidence are various, or, in other words, where the evidence is only of the probable kind, it is incumbent on the ſearcher after truth, to weigh well the evidence and arguments on both ſides, and to embrace that in favour of which the evidence preponderates. In the preſent inſtance, then, if we proceed upon this principle, that is, proportion our aſſent to the degree of evidence derived from the facts narrated, and the credible teſtimony of reſpectable practitioners, we cannot, I think, heſitate to conclude, that Pemphigus, when uncombined with any other affection, is a diſeaſe purely ſporadic, and not of a contagious nature.

Upon the whole, however, though the evidence deduced from the few facts already collected, muſt incline the balance, and determine the probability to one ſide, a philoſophical ſcepticiſm may yet perhaps dictate to us, the propriety of ſuſpending our judgement, and neither abſolutely affirming nor denying the contagious character of the diſeaſe, until, by a cautious induction from a ſufficient number of caſes and obſervations, we ſhall be enabled to form a poſitive concluſion on the ſubject.

SECT. III.

MEDICAL NEWS.

DR GUTHRIE of Peterſburgh, in a letter to Dr Duncan, gives him the following articles of information reſpecting ſome diſeaſes of the Ruſſians, and other medical ſubjects.

"I have lately ſeen a caſe at a friend's country-ſeat, which I preſume may be new to you, as it was to me, although we muſt both have heard of ſuch. Indeed the Abyſſinian Bruce relates his own caſe pretty diſtinctly.

"This was a woman with her hand and arm in a moſt violent ſtate of irritation, from the lodgement of the Lumbricus melitenſis, a worm not much thicker than a horſe-hair, which had entered her thumb whilſt at work in a marſhy ſpot, and was, when I ſaw her a day after the accident, on its way up the arm, with excruciating pain. I muſt own that I ſhould not have known the nature of the diſeaſe, if a fellow-peaſant had not immediately declared, that it was produced by the dangerous worm; which many of theſe people are acquainted with to their coſt, as an inhabitant of the ſtagnant pools and marſhes in that diſtrict, about ſixty verſts to the ſouth-weſt of Peterſburgh. I muſt farther acknowledge, that I was happy to hear the patient was to be inſtantly tranſported to another village, where a famous operator lived, well ſkilled in the art of extracting the venomous inſect; as I ſhould certainly have gone very awkwardly to work, if I had been obliged to operate. Though the ſimple peaſants perform it with ſucceſs and ſafety, gradually winding the worm round a quill, till the whole animal be extracted; a work of much patience and perſeverance. I was very ſorry that duty obliged me to be in town the ſame evening, a drive of ſixty verſts. It was therefore impoſſible for me to accompany the patient, though I was very deſirous of witneſſing this village-operation.

" But Ruſſia is peſtered with a ſtill more dangerous worm, the Furia infernalis. It is ſtill ſmaller than the former, not being thicker than a human hair.

" This infernal inſect, from its extreme lightneſs, is often carried up into the air with the duſt, by whirlwinds; and if it unfortunately falls on the uncovered part of a man or beaſt, it enters the fleſh in an inſtant, and ſoon proves mortal, if a remedy be not quickly applied.

" Our new Vice-Governor of Peterſburgh lately came down from Siberia, where he was Commandant of a fort. He tells me, that in the diſtrict of Nerchinſk, where he commanded, the peaſants, as well as their cattle, are often deſtroyed by an inſect falling upon them. To prevent this accident from proving fatal, the part is inſtantly ſcarified, and rubbed with a mixture of ſnuff and ſal ammoniac.

" This I am convinced muſt be the Furia infernalis; more eſpecially as he aſſured me, that the inſect was ſo very minute, that none of the peaſants had ever ſeen it when it fell upon them, and that they had no idea of its nature and form. But one circumſtance of his, recited, almoſt ſtaggered my belief, viz. that the carcaſe of an animal killed by this inſect, is almoſt as dangerous as the inſect itſelf. This phœnomenon I cannot account for in any other way, but by ſuppoſing, as the accident always happens in the hot months of the year, that a high degree of putridity is produced by the venomous worm, when the caſe proves fatal.

" I informed the Vice-Governor of the manner in which the Dalecarlian peaſants in Sweden treat the accident, in order that he might communicate their mode of cure to his Siberian acquaintance, which is merely applying to the part affected, a piece of ſweet curd. The inſect poſſibly prefers this to fleſh, and leaves the one for the other. I am, however, much affraid that this ſimple remedy will ſeldom be at hand in Ruſſia, as the peaſants are unacquainted with the uſe of rennet, but prepare a ſour curd by means of heat, throughout the whole empire; evidently taking its origin in the Tartar Koumis, and ſhewing them to be a people of Scythian extraction. They likewiſe ſeparate butter from milk by heat, inſtead of the churn; a curious circumſtance, probably unknown to you before.

"These two dangerous inſects are, however, not all which threaten the life of man in this empire. The ſouthern provinces are infeſted with a third, the bite of which is as mortal as that of the deadly rattleſnake, if the part be not inſtantly ſcarified, and rubbed with freſh butter. This is a ſpecies of crab-ſpider, the Phalangium acaroides, reſembling the tarantula, but rather thinner and ſmaller. It, however, kills and devours that formidable ſpider in a few minutes, which, when compared with it, is an innocent animal.

"Your acquaintance, Mrs Guthrie, lately returned from a tour, on account of health, along the north ſhore of the Black Sea. Among much important and curious information, ſhe gave me ſome account of the cure employed for the bite of this mortal ſpider, which finds many lurking places among the ruined buildings of the ancient Cherſoneſus, Taurica, or Crimea, laid waſte partly in the laſt Turkiſh war. It is a curious fact, that animal oil counteracts the venom of the ſpider tribes, as vegetable oils do the venom of ſerpents. I ſuſpect, however, that either of them would counteract both poiſons; indeed, I think we have a proof of animal oil acting wonderfully on ſerpents, in the anecdote related by Bruce, when the deadly Ceraſtus, or viper of the Nile, turned away its head from the oily breaſt of the prime miniſter of Fenaar, when he careleſsly took it up in his hand, and applied it to his naked boſom, to ſhew Mr Bruce how innocent it was to men of his colour, whoſe very ſkin ſickened the animal, and made it avoid all contact.

"I have lately received a very intereſting viſit from Mr John Fraſer, collector of exotics, Sloan Square, Chelſea, who brought over a number of curious plants for our Emperor. This indefatigable botaniſt, who has been five times in America, to explore the woods of that vaſt continent, informs me, that during thoſe excurſions, he got notice of ſeveral medicinal plants, with which the Indians, and their ſcholars the ſettlers, perform wonderful cures. For example, he aſſured me, that he was ſaved in the laſt ſtage of dyſentery, when he had given up all hopes of life, by a decoction of the roots of the vaccinium myrtillus, or Bilberry, with the addition of two glaſſes of burnt gin to a pint, which he drank, evening and morning, for three weeks, after which he found himſelf completely cured. This happened on the banks of the river Savanna, in Georgia.

" He likewiſe told me, that a ſmall ſpecies of wild ſun-flower, growing ſpontaneouſly in the woods of America, is uſed with great ſucceſs by the natives, for the cure of cancer, both in decoction, and in the form of an ointment with hogs-lard.

" Laſtly, he told me, that in Carolina, Georgia, and Eaſt Florida, but more eſpecially in the laſt-mentioned country, where bilious intermittents are equally frequent and dangerous, the inhabitants uſe, with wonderful ſucceſs, as a tonic and febrifuge, the Eupatorium perfoliatum, or Thorough wort. And as it can be given with equal ſucceſs during the paroxiſms and intermiſſions, they prefer it to the Peruvian bark.

" There is a curious fact in the natural hiſtory of man, which I got from Commodore Billings, who commanded the late northern expedition, fitted out by Catharine the Second, and which was confirmed to me by the ſurgeon's mate, and ſecretary who attended him. In my experiments on the congelation of mercury, publiſhed ſome years ago, at Peterſburgh, in French, and by Mr Cox, in the third volume of his Northern Tour, I never could produce a greater degree of cold than 36 deg. below o. of Reaumur, though aided by 20 deg. below o. of natural cold, and by all the power of frigorific mixtures then known. Nay, I ventured to ſurmiſe, that there probably did not obtain a much greater degree of cold in the habitable parts of the globe, than 36 deg. below o. How much, then, was I ſurpriſed to hear Captain Billings aſſert, that ſome ſpirit-thermometers which he had with him, graduated according to Reaumur's ſcale, were often as low as 40 deg. below the freezing point of water, that is, 8 deg. below the freezing point of mercury. And once or twice he obſerved them at 42 deg. below the freezing point of quickſilver.

During this ſevere cold, the Nomade Tchutſki, who were conducting him along the coaſt of the frozen ocean, in ſledges drawn by rein-deer, encamped every night on the frozen ſnow in low tents, which they quickly formed with the ſkins of rein-deer, ſpreading ſome of them on the ſurface of the ſnow, on which they all ſlept; and he aſſured me, that ſo far from ſuffering from cold during the night, the heat was ſo exceſſive in thoſe fur-tents, where from ten to fifteen ſlept together, according to its dimenſions, that no one could bear even a ſhirt, but all lay in a violent perſpiration, naked as they were born, till dawn of day, without the aid of fire, excepting a train-oil lamp, which lighted each tent.

During all this journey of nine months from Tchutſki Nos, the Aſiatic promontory on the American ſtraits to Yakouts, they lived on frozen fiſh, and ſometimes a little rein-deer fleſh, but always without ſalt, which the Glacial wanderers, his conductors, made Billings underſtand was poiſon in their climate; and they threw away a quantity he had brought with him from his frigate, with expreſſive marks of abhorrence. The ſcurvy ſeems to be a diſeaſe unknown to the Tchutſki, who are a ſtrong healthy race of men, of tall ſtature, and remarkably bold and warlike. Billings and his ſuite likewiſe eſcaped ſcurvy, during their long frigid journey. This I think a curious fact, but whether it was owing to their abſtinence from ſalt, I will not take upon me to determine. I ſuſpect, however, that their warm fur-dreſs during the day, and the violent perſpiration at night, muſt have contributed greatly to ward off putreſcence.

"A circumſtance in the moral hiſtory of the polar race of men, is, I think, likewiſe worth relating; that is, their acknowledging no other kind of ſuperiority in the human ſpecies, than what belongs to ſtrength, figure, and ſize. And according to theſe ideas, they treated as chief of Billings's party the ſurgeon's mate, a tall ſtout youth, upwards of ſix feet high. They allotted to the Captain himſelf, merely that conſequence to which his ſtature and figure, in their opinion, entitled him; whilſt his ſecretary, an Engliſhman, of a very diminitive ſize, was treated by them as a ſlave, and obliged to perform all the menial offices during the journey. All remonſtrances through their interpreter, to permit the Captain to aſſume any command of his own crew, were long in vain. By degrees, however, they began to relax, from obſerving the reſpect paid to him by his officers and men, and probably ſtill more from remarking, that he had the diſtribution of the tobacco, &c. of which they were extremely fond. That fund of riches, in the eyes of his conductors, Billings told me, would have probably coſt the lives both of himſelf and his ſuite, if he had not made the ſavages imagine, that his frigated eſtined to winter at Kamtſchatka, was to remain where he had diſembarked, till his firſt lieutenant got a letter from their commander, by the return of his eſcort, mentioning his ſafe arrival at Yakouts. And as the ſhip was left at anchor, near their principal village, they ſtood in awe of the vengeance that might be taken.

"I doubt if you are informed of the reſult of this northern expedition, as nothing has been publiſhed on the ſubject, excepting perhaps that our late chaplain, Mr Took, may have printed in ſome ſhape the ſhort notes I ſent him two years ago. I believe I already mentioned in Dr Anderſon's publication, the Bee, that Billings, in deſcending the river Kuluma, into the frozen ocean, found only a certain ſpace open by the fall of the warmer waters of that river, into the Glacial Sea; and all the reſt a ſolid maſs of ice, of immenſe thickneſs; ſo that the eternal icy barrier of Phipps and Cook, being but too well aſcertained, he proceeded by land to the ſea of Ochotſki, there to embark in the frigate built for his American voyage, and to execute the reſt of his inſtructions; the firſt part of them having been completely foiled, no paſſage being found from the mouth of the Kuluma, round to Kamtſchatka by ſea.

"Another circumſtance of his expedition, intereſting to the public in general, is the final opinion he formed, that the long ſought for paſſage, between the continents of America and Aſia, does not exiſt. The facts which determined Billings to adopt this opinion, are, that while coaſting the frozen ſea in his return to Ruſſia, as mentioned above, his conductors from Tchutſki informed him, that every year there came over from America to the Ruſſian ſide, immenſe herds of rein-deer, regularly in the month of May, and returned as regularly in Auguſt, the very time when the ſea muſt have been open after the conſtant action of the ſun, during their ſhort ſummer, if it be ever open. But the Captain is convinced, ſince he had an opportunity of examining the amazing thickneſs of the ice, in his laſt journey, which no man but himſelf ever made, that a ſummer of many years duration, without intervening froſt, would not be adequate to thaw the monſtrous icy cruſt that covers the frozen ſea, in the very part where a paſſage was expected to be found.

"The curious fact of theſe deer of paſſage ſeems to ariſe from the animals flying the moiſt American woods during the ſummer-months, when they are full of troubleſome inſects, to graze at their eaſe on the high open plains on the Aſiatic ſide, till their tormentors are no more. Then they return to America, probably for the protection of the woods in winter. The paſſage is rendered eaſy to them from ſome intervening iſlands, where they find plenty of moſs, the Lichen rangiferinus, their common food."

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Mr David Paterson, now ſurgeon in Montroſe, has favoured us with a copy of a letter on the good effects of nitrous vapour, which, though not publiſhed, has been printed by the Navy Medical Board, with the ſanction of the Right Honourable the Lords Commiſſioners of the Admiralty, for the uſe of the navy; and we flatter ourſelves,

ſelves, that no inconſiderable benefit may ariſe from the important facts which it contains being generally known.

Copy of a Letter from Mr David Paterſon, *a Surgeon in his Majeſty's Navy, and late Surgeon to the Priſoners of War at Forton, to the Commiſſioners for taking care of Sick and Wounded Seamen, &c.*

LAST winter, when I had the charge of Forton hoſpital, although in the midſt of very fatiguing duty, and engaged as I was in making ſome favourite experiments of my own, yet, being extremely anxious to acquire ſome practical knowledge of Dr J. Smyth's Nitrous Vapour, I failed not, after receiving your authority, to put his fumigating plan in execution as extenſively as it was poſſible ; and, at the ſame time, to note down the phænomena, as they occurred, with as much accuracy as my leiſure-time would permit. And now, in order that you may ſee, in the faireſt points of view, ſome of the effects of the vapour reſulting from that highly ingenious, and very ſalutary proceſs; and alſo with the view no leſs of doing juſtice to Dr J. C. Smyth, than of rendering his Nitrous Vapour more extenſively uſeful in the Navy, Army, &c.; I beg leave to communicate to you the contents of the following pages; which, ſhould they appear to you in any degree calculated to be ſerviceable in promoting the welfare of theſe realms, I humbly requeſt you will be pleaſed to lay before the Right Honourable the Lords Commiſſioners of Admiralty.

In purſuing my preſent plan, I ſhall, after giving you a ſhort, but not imperfect, account of the method I followed in fumigating the wards of the hoſpital, ſtate, in a faithful manner, ſuch facts, whether of a general or of a particular nature, as aroſe from the trials that were made; after which, by moſt humbly offering a few obſervations connected with the ſubject, I ſhall conclude my letter.

Every evening a certain number of wards were fumigated, each by means of three pipkins, for an hour; the galley-pot in each pipkin containing the quantities of pure nitre in powder, and concentrated vitriolic acid, as directed by Dr Smyth [*]. Three perſons, each carrying one fuming pipkin, went round a ward, following one another at ſome diſtance, and holding the pipkins under each bed, for a conſiderable time, as they went along; and they continued doing ſo as long as the fumigation laſted. The wards, by opening the windows and doors, were afterwards filled with atmoſpheric air.

The vapour, proceeding from the decompoſition of the nitre, by means of the concentrated vitriolic acid, was in ſuch great quantity, that a ward 57 feet by 20, and 10 feet 6 inches high, was filled with it, by means of three pipkins, in the manner I have mentioned, in the ſpace of fifteen minutes.

On the wards being filled with Nitrous Vapour, ſome of the patients who laboured under affections of the lungs were ſeized with fits of coughing; none of them, however, to any great degree. With little or no exception, the patients in the wards that were fumigated, bore the vapour without feeling any diſagreeable effect from it. If, indeed, a pipkin was accidentally held very cloſe to the mouths of any of the patients, which, from awkwardneſs, was ſometimes the caſe, coughing was immediately produced, and in one inſtance vomiting was occaſioned. Theſe circumſtances, however, did not prevent the patients from becoming, in a ſhort time, very fond of the fumigating buſineſs. For my part, I frequently remained in a ward during the whole time of the fumigation, often indeed with a fuming pipkin in my hand, without experiencing any diſagreeable effect whatever. The fume was to me pleaſant. When, during the fumigation, I remained in a ward, I always wore black cloaths, which, even after being repeatedly expoſed to the Nitrous Vapour, were not in the leaſt either ſtained, or changed from black to a brown colour.

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[*] For a more particular hiſtory of the proceſs, vide Dr J. C. Smyth's letter to the Right Hon. Earl Spencer, &c. &c. &c. containing an account of the experiment made on board the Union hoſpital-ſhip, to determine the effect of the Nitrous Acid in deſtroying contagion, &c.

In the mornings, particularly in dry weather, the wards that had been fumigated the preceding evening, even although they had been waſhed early in the morning, and the windows kept open, had a very agreeable ſmell; much more pleaſant than that which was experienced during the fumigation. By this agreeable odour, in the mornings, I was able to judge whether due pains had been beſtowed, the preceding evening, in fumigating the wards.

One Dyſentery-ward, one Fever-ward, and one Surgery-ward, containing the worſt kind of ulcers, were, at firſt, the places filled every evening with the nitrous vapour; but, as the good effects reſulting from the fumigation were to me very obvious, I ſoon uſed it more extenſively. The patients, in general, who laboured under old dyſenteries, many of them contracted in the Weſt Indies, ſeemed to be greatly relieved; the fevers, which were of no uncommon genus, and which were in their nature very mild, ſoon diſappeared, without exhibiting any ſymptoms of typhus; and the ulcers, inſtead of further degenerating or ſpreading, put on a favourable appearance, and healed *.

It is, I preſume, of no ſmall conſequence to obſerve, that, excepting ſome marked caſes of dyſentery, among the ſervants of the priſon and hoſpital, in the months of Auguſt and September, before the arrival of the priſoners from the Weſt Indies, and one caſe of typhus (in ward 18), with now and then a caſe of ſmall-pox, among the Weſt Indians, after their arrival, there was not any contagious febrile diſorder that made its appearance within the walls of the hoſpital, while I had charge of it, notwithſtanding the many ſources of contagion to which, in my opinion, all in and about it were expoſed. During the laſt five months of my time, no fewer than 1686 patients were admitted into the hoſpital, as may be ſeen by the hoſpital-books.

The following table ſerves to ſhew, at one view, the higheſt number of patients in the

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* Ultimately, a great number of the old dyſenteries, where the patients were not far advanced in life, did well.

the hoſpital, the number diſcharged and dead, weekly, for four weeks before, and ſix weeks after the nitrous vapour was firſt uſed, viz. from the 16th of October, to the 26th of December 1796. To include a greater ſpace of time would be improper; becauſe, before the 16th of October, there were but few patients in the hoſpital; and becauſe, after the 26th of December, there were a great number of extremely bad caſes of gangrenous feet, pneumonia, &c. received into it, from Portcheſter hoſpital, and from the Vigilant and Captivity priſon-ſhips.

TABLE.

Before the Nitrous Vapour was uſed.				After the Nitrous Vapour was uſed.			
Weeks.	Higheſt Number in the Hoſpital.	Number diſ-charged.	Number dead.	Weeks.	Higheſt Number in the Hoſpital.	Number diſ-charged.	Number dead.
1	223	2	8	1	340	27	6
2	372	4	21	2	332	7	5
3	371	0	13	3	342	11	8
4	369	1	9	4	340	8	4
				5	486	12	1
				6	539	63	5
		7	51			128	29

After theſe general obſervations on the Nitrous Vapour, I ſhall humbly beg leave to offer the following caſes, in which it was, undoubtedly, uſed with very remarkable ſucceſs.

CASE I.

JEAN LOUIS, French priſoner, of colour, eighteen years of age, from the Weſt Indies, was admited into the hoſpital on the 28th of October 1796, for an ill-conditioned ulcer on the inferior and interior part of the right leg. After he had been ſome time in the hoſpital, the ulcer began to put on a favourable appearance, and was ſoon conſiderably diminiſhed in its ſize, merely by means of ſimple dreſſings.

On the 29th of November, however, the ulcer, according to the common phraſe, became foul; and, by next day, it had ſpread to ſuch a degree, that it was nearly as extenſive again as it ever had been, attended with very acute pain, and with a very copious, thin, dark-coloured fœtid diſcharge. The patient's pulſe, at this time, was 120, tongue clean, appetite impaired, belly open, ſleep much diſturbed.

31ſt. From the 29th to this time, a common poultice, thrice a-day, was the only application; but now, in addition to the poultice, the ulcer was dreſſed with the powder of Peruvian bark; a cooling medicine, with an opiate at bed-time, was ordered; and a vegetable diet, with milk, was enjoined.

2d December. The ulcer ſtill more extenſive than it was on the 31ſt ult. It now extended from the tarſus ſix or ſeven inches upwards, and from the tibia more than half round the leg; it was ſtill in a ſloughing ſtate, with high reflected edges. The other ſymptoms much the ſame as before. Finding that the plan hitherto purſued had not produced any good effect, either on the ulcer or on the ſyſtem, the whole of it, excepting the poultice, was abandoned, and the Nitrous Vapour adopted. The ward in general, and the bed of the patient in particular, were carefully fumigated, the ward once, the bed twice a-day.

3d. The ulcer had ſtopt ſpreading, and in ſome places looked clean. Such a ſudden change was to me aſtoniſhing. Pulſe now 110, tongue clean, belly open, ſlept better on the night of the 2d than for ſome time before, notwithſtanding the omiſſion of the opiate.

4th. The ulcer was clean, and diſcharged good matter. The patient felt himſelf comfortable. Pulſe about 90.

6th. The diſcharge continued to be good,

and

and the ulcer had made conſiderable progreſs in healing. The patient felt himſelf perfectly eaſy, and his health was already very much mended.

The fumigation was continued until the 26th of December; from which time, owing to a want of materials, it was diſcontinued until the 11th of January 1797. The ulcer, during the time the fumigation was uſed, and even to the 1ſt of January 1797, continued to heal kindly and rapidly; but, at that period, it again became foul and ſloughing, and was ſoon as extenſive as before. The appetite was again impaired; pulſe 120, and ſmall; belly open, the patient much weakened and emaciated. Half a drachm of Peruvian bark, thrice a-day, and eight ounces of wine in the twenty-four hours, were ordered; and the ulcer was dreſſed twice a-day with the powder of Peruvian bark and common poultice. An opiate was occaſionally allowed at bed-time. This treatment was perſevered in until the 11th, when, having experienced no good effects from it, it was diſcontinued, and recourſe again had to the Nitrous Vapour and common poultice, as on the 2d of December 1796.

12th. The ulcer had ſtopt ſpreading, and in ſome places had begun to clean. The pulſe was leſs frequent, and more full; and the patient was, in every reſpect, better, and more comfortable. The Nitrous Vapour, &c. were continued.

13th. The ulcer was perfectly clean, with florid granulations, and with about the eighth of an inch of new ſkin round the edges. The plan was continued.

20th. The ulcer looked very healthy, and was contracting rapidly. The plan was continued.

5th February. The ulcer had contracted more than one half. The plan was continued.

12th March. The ulcer was nearly healed, (not ſo much as the breadth of a ſixpence being open), and looking healthy. On this I finiſhed my duty, and conſequently my obſervations, at Forton hoſpital.

I have here to obſerve, that about the 1ſt of January 1797, all the ulcers in the ſame ward (N° 14) with the above, were more or leſs in a bad ſtate; and that they all, about one and the ſame time, began to put on a favourable appearance; and alſo that, in a ſhort while, many of them healed. Likewiſe, it is neceſſary to obſerve, that particular attention was paid all along to cleanlineſs and ventilation.

CASE II.

La Grenade, French priſoner, aged 26 years, from the Weſt Indies, was admitted into the hoſpital on the 16th of December 1796, for chilblains. In the end of February 1797, an ulcer broke out on his leg, which became very foul and ſloughing, and did not yield to common remedies. On the 7th of the following March, the Nitrous Vapour was uſed, exactly in the ſame manner as in the preceding caſe; and by the 12th the ulcer was perfectly clean.

CASE III.

Elie Double, French priſoner, aged 22 years, from the Weſt Indies, was admitted into the hoſpital on the 28th of October 1796, for an ulcer on the anterior and middle part of the left leg. By the middle of February 1797, the ulcer was cicatriſed, but with a conſiderable protuberance remaining over that part of the tibia, as if the perioſteum, and even the bone itſelf, had been in a diſeaſed ſtate. About the end of February, the cicatrix became inflamed, ſoon ſuppurated, and degenerated into a foul ſloughing ulcer, which, inſtead of yielding to any of the various applications, got worſe and worſe every day. From the end of February, (I cannot exactly tell the day), cataplaſms of different kinds, myrrh, and Peruvian bark, were tried externally; and wine, Peruvian bark, opium, &c. were adminiſtered internally. At the ſame time, great attention was beſtowed in keeping the ward extremely clean, and thoroughly ventilated. Finding not only that no good effect was produced by any of theſe means, but even that the ulcer, the ſound parts being ſtill in a mouldering ſtate, grew more and more extenſive, I came to a determination, conſidering myſelf ſufficiently authoriſed,

thoriſed, from the experience I had had, to make trial, in this untoward caſe, of Dr J. C. Smyth's Nitrous Vapour. Accordingly, on the 7th of March, the fumigation was put in practice, in the ſame way as in the foregoing caſes; and it is with heart-felt pleaſure I relate it, by the 12th, that was in five days time, and on that day I finiſhed my duty at Forton, the ulcer was perfectly clean and healthy.

CASE IV.

Francois, a negro French priſoner, age unknown, was admitted into the hoſpital on the 26th of January 1797, for a wounded little-finger. On examining the wound, I found that the laſt bone and the ſurrounding teguments were the parts moſt materially injured. The bone was fractured, and the ſoft parts were contuſed to a very great degree, with a ſmall lacerated wound at the tip of the finger. Deeming it neceſſary, I immediately amputated the limb at the joint formed by the ſecond and laſt phalanges. The ſtump, the bone being well covered,

vered, and the ſoft parts looking healthy, had all the appearance of doing well during the firſt fortnight; but, unfortunately, at the expiration of that period, it began to put on a very unfavourable aſpect. Inſtead of the diſeaſed parts being ſinuous, or having, what is perfectly underſtood in ſurgical language, a glaſſy appearance, which ſometimes indicates a diſeaſed bone, they became enlarged to a prodigious degree, reflecting very conſiderably, ſo as to reſemble a ball on the end of the ſtump; and at the ſame time appeared foul, diſcharging a dark thin fœtid matter. In this ſtate, Peruvian bark, opium, &c. were tried, as alſo cataplaſms, but to no purpoſe. In the end of February, recourſe was had to the Nitrous Vapour; and, by means of it, in ſix days time the ulcer was perfectly clean.

CASE V.

Bastern, a negro French priſoner, age unknown, was admitted into the hoſpital on the 28th of January 1797, for an ulcerated toe. This caſe was very ſimilar to the finger of which I have juſt taken notice, and, like it, after various ineffectual applications, was cleaned, and put ſeemingly in a healthy ſtate, by means of the Nitrous Vapour uſed according to the manner I have already related. The four laſt caſes were in the ſame ward, No 4.

Having, with reſpect to Dr J. C. Smyth's Nitrous Vapour, agreeably to my promiſe, finiſhed the moſt important part of my experiments, with the phænomena reſulting from them, in order to ſhew you, that that vapour, under proper management, is capable of producing very happy effects on the human frame, I ſhall now moſt humbly offer a few obſervations which appear to me to be connected with the ſubject.

And to proceed: I am in great hopes that the facts detailed in theſe pages, while they ſerve as ſo many proofs of the utility of Dr J. C. Smyth's fumigating plan, will, at the ſame time, anſwer the happy purpoſe of not only removing the ill-grounded fear of Dr Trotter*, and of convincing him, as well as thoſe who think as he does, that no danger is to be apprehended from the combination of azote with the Nitrous Vapour; but alſo of conquering the prejudices of thoſe gentlemen who imagine, (for ſome highly reſpectable medical practitioners have lately mentioned to me their apprehenſions), that that vapour, from its being loaded with vitriolic acid, muſt be intolerable to the lungs, and of courſe highly pernicious to perſons ſubjected to its influence.

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* Vide Dr Trotter's Medicina Nautica.

With regard to the bad or deleterious effects of the Nitrous Vapour, I cannot ſay, from experience, that I am acquainted with any of them. The trials that I made of that vapour were on a great number of diſeaſed perſons, who, although crowded together within the walls of an extenſive hoſpital, and from that circumſtance, as well as others of at leaſt equal moment, expoſed to the influence of noxious effluvia, were obviouſly, in many inſtances, as already mentioned, benefited by its ſalutary effects. Many patients were cured; others were put in the fair way of being cured. And I muſt add, for it is not, I preſume, altogether improbable, that, by means of the Nitrous

Vapour, with other no leſs important meaſures which I adopted, and inceſſantly followed when in my power, the patients who were under my charge in Forton hoſpital, were preſerved from the attacks of contagious fever. I have ventured to ſay, with other no leſs important meaſures, becauſe I am well aware, as Dr J. C. Smyth undoubtedly is, that, without the moſt ſtrict attention to cleanlineſs, and to the circulation of pure or atmoſpheric air, neither the Nitrous Vapour, nor any thing adminiſtered with ſimilar intentions, can prove ſo efficacious as we could wiſh, in preventing or putting a ſtop to contagious fever, as well as other diſeaſes, though perhaps not ſo immediately, yet ultimately as fatal. I am here under the neceſſity of obſerving, having forgot to do it in the proper place, that the ſolitary caſe of Typhus, which, as before mentioned, was in ward 18, did not originate in the hoſpital, but in the Captivity priſon-ſhip. What the nature of the diſorder had been primarily, I am at a loſs to ſay. The patient died, and fortunately ſo did the diſeaſe; for I ſaw not another fever of a ſimilar nature in the hoſpital.

Although no contagious fever (I mean typhus, or what ſome authors have called Jail-fever, others Hoſpital-fever, &c.) prevailed, during my time, in Forton hoſpital, I cannot ſay poſitively, that the Nitrous Vapour ſolely prevented ſuch a fever from prevailing. All that I can ſay is, that the circumſtance of no contagious fever having prevailed in Forton hoſpital, during my time, may be conſidered as being of a very ſingular nature; more eſpecially when we take into our view the vaſt number of patients, in the moſt filthy ſtate, from the Weſt Indies, &c, that were received. The very particular attention that was paid to the patients on their being received, in ſtripping them of all their cloaths, in bathing them, in ſhaving their heads, or cutting their hair very ſhort, in burning all their cloaths, and alſo in keeping the hoſpital at all times extremely clean, and thoroughly ventilated, may, it is probable, have contributed not a little towards preventing contagious fever. And further, another circumſtance which, perhaps, had operated very powerfully in aſſiſting to obviate contagious fever, and which deſerves to be very particularly remarked, was the changing of the wards as frequently as it was poſſible, and that according to the nature of the complaints they contained: for inſtance, wards that contained convaleſcents, and alſo thoſe that contained ſlight or chronic diſeaſes, were changed frequently; thoſe that contained febrile diſeaſes more frequently; and thoſe that contained very bad ſurgical caſes, moſt frequently. By the changing of wards, I mean the removing of the patients from one that they had occupied for ſome time, to another that was perfectly purified.

When a ward, of whatever deſcription, was changed, it was firſt emptied, by the patients being removed into another, and by its bedding being ſent to be baked, fumigated, or waſhed; then it was without loſs of time fumigated by means of ſulphur; then white-waſhed; then its cradles cleaned, and waſhed with vinegar; then the floor of it thoroughly cleaned; and, laſtly, its windows on the one ſide, and its ſcuttles on the other, were kept open, always when the weather would permit, until it was occupied by patients, which, if the ſtate of the hoſpital admitted, was not before eight days had expired. Such regulations as the above, ought, in my humble opinion, to be conſtantly and very particularly obſerved, by all medical gentlemen who have the immediate charge of hoſpitals for priſoners of war: many of them, I preſume, might with propriety and utility be obſerved in any hoſpital; and, in concluding this ſubject, I beg leave humbly to ſuggeſt to you, that no hoſpital ought to be full, but, on the contrary, that there ſhould always be, in all, according to their different ſizes, two, three, four, or more wards left empty, for the very ſalutary purpoſe of changing.

Cleanlineſs, ventilation, and changing of the wards, whether with the view of obviating or removing diſeaſes, are, in all hoſpitals, as well as in all places where priſoners of war are confined, &c. abſolutely neceſſary: where they are obſerved, medicines will become leſs needful; and when needful, they will, in their operation, be more effectual; but where they are neglected, the phyſicians and ſurgeons will be ſubjected to the very unpleaſant trouble of giving their attendance, and of preſcribing, to very little purpoſe. Warmth ought alſo to be attended to.

In ſuch eſtabliſhments as Forton, cleanlineſs, a free circulation of air, proper diet, &c. ought, agreeably to the very particular orders which you iſſue, to be moſt rigidly attended to, from one end of theſe eſtabliſhments to the other. But, I am afraid orders are not always rigidly executed. The unpardonable neglect of ſervants, in not executing, with promptitude and ſcrupulous punctuality, the orders with which they are intruſted, is to be lamented, but, I fear, not to be on all occaſions either prevented or corrected. From what I know of the eſtabliſhments in queſtion, I ſhall venture to ſay, that, were they always to be properly conducted by the ſervants who have the immediate charge of them, we ſhall hear leſs frequently of the priſoners. &

&c. falling a prey to contagious fever, than we have hitherto done. This is a ſubject, however, with which, at preſent, I ſhall not further concern myſelf, excepting to make the following obſervation, which is, that when due care is not taken, in the firſt inſtance, to prevent contagion from taking effect, the uſe of Dr J. C. Smyth's Nitrous Vapour becomes, undoubtedly, the more particularly neceſſary: and I am extremely ſorry to think that Dr Smyth's plan, as well as others equally well intended, ſhould not always be put in execution, more eſpecially in caſes of emergency, with that facility, with that eagerneſs, with that candour, which duty, juſtice, and humanity continually require.

Although I have, in the courſe of theſe obſervations, laid very conſiderable ſtreſs on cleanlineſs, ventilation, changing of wards, &c. yet I would not, by any means, wiſh it to be ſuppoſed that I have done it with the view of ſuperſeding the uſe of the Nitrous Vapour; on the contrary, while on the one hand I am ſenſible that the Nitrous Vapour cannot, without cleanlineſs, ventilation, changing of the wards, &c. be ſo efficacious as we could wiſh in putting a ſtop to contagious fever, I am, on the other hand, no leſs ſenſible, that fever, when raging to a violent degree, cannot be exterminated by means of cleanlineſs, ventilation, &c. without the aſſiſtance of ſome other means. With reſpect to hoſpitals, ſhips, priſons, &c. where people are crowded together, where the introduction of contagious fever is dreaded, or where it actually prevails, the Nitrous Vapour, with due attention to cleanlineſs, ventilation, &c. may at once, I preſume, not only be conſidered the moſt convenient, the moſt elegant, and the moſt ingenious, but alſo the moſt efficacious remedy, for the purpoſe of counteracting different ſpecies of contagion, that has yet been offered to the public.

Further, although cleanlineſs, ventilation, and the changing of wards, very ſtrictly attended to, might, in a very great meaſure, prevent contagion from taking effect, or from ſpreading extenſively; yet, ſuppoſing them to be attended to as ſtrictly as, from the nature of things, is poſſible, they could not, I am too much afraid, deſtroy contagion, when prevailing in an extenſive hoſpital, &c. For example, let us ſuppoſe only five or ſix hundred patients confined in Forton hoſpital, and labouring under contagious fever; and let us alſo ſuppoſe it neceſſary, for the ſake of cleanlineſs, and of putting a ſtop to the contagion, to completely ſhift all theſe patients once, perhaps many of them twice, and ſome of them even thrice, every day; how, give me leave to aſk, would it be poſſible to furniſh ſuch a great number of patients ſo frequently with the clean things required? For my part, I am fully perſuaded, that it would prove difficult, ſo extremely ſo, indeed, that it would amount even to an impoſſibility. With reſpect to ventilation, has it not been found, even when it has been attended to very particularly, to be, without the aſſiſtance of other means, inadequate to the ſpeedy deſtruction of contagion? And, with regard to the changing of wards, were it ſufficient of itſelf to deſtroy contagion, might it not, I ſhall ſay ſometimes, from the number of patients being equal, nay even more than equal, to all the wards of which the hoſpital conſiſts, be utterly impracticable? Other examples, and other queries to the ſame effect, were they not deemed ſuperfluous, might be advanced; conſidering, then, the buſineſs in this point of view, does it not become a duty incumbent on us to look out for, and to try other means more active, and more diffuſive, which, with the aſſiſtance of cleanlineſs, ventilation, changing of wards, &c. may be employed for the purpoſe of more ſpeedily, and more effectually deſtroying contagion? and may not the Nitrous Vapour of Dr J. C. Smyth, as I have already mentioned, be deemed, of all other remedies extant, the moſt convenient, the moſt elegant, the moſt ingenious, and the moſt efficacious for anſwering the wiſhed-for purpoſe, whether at ſea or on ſhore?

The extraordinary effects which we have ſeen the Nitrous Vapour produce in caſes of putrid ulcers, are facts of the utmoſt importance to mankind, and certainly deſerve the moſt ſerious attention of medical practitioners. They not only ſhew, in the moſt ſatisfactory manner, the power of that vapour in ſuch caſes, but alſo point out, in my humble opinion, the probability of its having, in a ſimilar way, as ſalutary a power in contagious fever, and in many other diſeaſes proceeding from other ſpecies of contagion. This opinion may, perhaps, ſeem ſingular; but I ſhall endeavour to evince its conſiſtence with reaſon and experience.

In hoſpital-practice, it has been frequently obſerved, not only by me, but by other medical practitioners, that all the ulcers of patients in the ſame ward have on a ſudden, and nearly at one and the ſame time, changed from, apparently, an healthy, to a foul, ſloughing, or putrid ſtate. I have beſtowed conſiderable attention in obſerving this change, and, in the courſe of my practice, have been able to make the following remarks, which I ſhall here arrange as they ſtand among my memorandums.

That firſt one ulcer degenerated, then another, and ſo on, until all the ulcers in the ſame ward had taken on a ſimilar diſpoſition.

That thoſe ulcers neareſt the one which

firſt degenerated, were ſooner affected than thoſe at a greater diſtance.

That this lamentable change did not happen in all the ſurgical wards at the ſame time.

That the patients, when their ulcers were in this degenerated ſtate, laboured more or leſs under ſymptoms of fever, ſuch as a frequent ſmall pulſe, unnatural heat, ſometimes chillineſs, dry ſkin, loſs of appetite, &c.

That common dreſſings, common poultices, carrot poultices, turnip poultices, myrrh, Peruvian bark, applied to the ulcers, had no good effects.

That Peruvian bark, wine, opium, given internally, had, I thought, inſtead of good, bad effects.

That in one caſe yeaſt was tried, both internally and externally, but the diſeaſe evidently gained ground under the courſe.

That the Acetum Nitroſum *, whether uſed internally or externally, ſeemed to have good effects.

That

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* Vide Paterſon on Scurvy.

That the changing of the wards had always good effects.

That the Nitrous Vapour, with the like attention to cleanlineſs and ventilation as was in common beſtowed, had, without changing the ward, as in the five caſes mentioned, as well as in many others, effects ſuperior to thoſe reſulting from the changing of the ward, without the uſe of the Nitrous Vapour.

That the Nitrous Vapour had not the like good effects, without cleanlineſs and ventilation, as with them.

From theſe premiſes, I have thought it warrantable to draw the two following concluſions:

1. That ſuch a degeneration of ulcers, in hoſpitals, from apparently an healthy to a foul, ſloughing, putrid ſtate, can only be accounted for on the principle of contagion.

2. That the Nitrous Vapour, with due attention to cleanlineſs, ventilation, changing of the wards, &c. is ſeemingly the remedy, the beſt calculated for preventing, or ſpeedily deſtroying that contagion; and from this naturally ariſes the following query:

As, under ſuch regulations, the Nitrous Vapour has ſuch great power in preventing or deſtroying one ſpecies of contagion, may it not, under the ſame regulations, be equally powerful in preventing or deſtroying other ſpecies of contagion?

I muſt here obſerve, that the ſecond concluſion does not exclude the uſe of other medicines. Suitable remedies, both internally and externally, uſed at the ſame time with the Nitrous Vapour, will, no doubt, forward the cure. But, as theſe pages are intended for the purpoſe of pointing out ſome of the effects of Dr J. C. Smyth's Nitrous Vapour, and not as a treatiſe on ulcers, I cannot, with reſpect to the latter, make, with any degree of propriety, an attempt on either the vindication of cure or remedies.

On the preſent ſubject, I might, to what has been advanced, add many more medical obſervations, were I not of opinion, that, after the experiments of Dr Smyth *, they would

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* Vide Dr J. C. Smyth's Letter to the Rt Hon. Earl Spencer, &c. &c. &c.

would appear ſuperfluous; and I might, with equal propriety, have recourſe to chemical reaſoning, were I not prepoſſeſſed with the idea, that, conſidering what has been already ſaid reſpecting Dr Smyth's Nitrous Vapour, by that very ingenious chemiſt, Mr Keir of Birmingham*, it would be extremely preſumptuous.

On the whole, and to conclude, I cannot help being of opinion, as well from the facts with which Dr Smyth has favoured the public, and from what Mr Keir has advanced, as from my own experience, that very great benefit muſt reſult to mankind from the *proper uſe* of the Nitrous Vapour, on board of ſhips, in hoſpitals, in priſons, in all places where people may be crowded together, and even in private families, in preventing and in putting a ſtop to contagion, as well as in mitigating and removing other diſeaſes, in which other medicines would not perhaps have the like good effects. And therefore, I moſt ſincerely wiſh that the plan of Dr J. C. Smyth may be univerſally adopted, and that it may, for the good of our navy and army, for the honour of our country, and for the benefit of mankind, be practiſed by medical men, and others, without their conceiving any prejudice againſt either it or its ingenious author.

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* Vide Dr J. C. Smyth's Letter to the Rt Hon. Earl Spencer, &c. &c. &c. Appendix.

Montroſe, Auguſt 12. 1797.

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The proceedings of the Aſſociation for promoting the diſcovery of the interior parts of Africa, containing an abſtract of Mr Park's account of his travels and diſcoveries, have lately been printed at London, for the Aſſociation. From this account, there is reaſon to hope, that the public will ſoon be favoured with a narrative of Mr Park's progreſs and diſcoveries, drawn up from the memorandums which he kept. But even from this epitome it appears, that the intelligence he gives is intereſting and important, and that the knowledge he has acquired cannot fail to promote, in an eminent degree, the views and purpoſes of the Aſſociation.

Some of the vegetable productions which

Mr Park has diſcovered in Africa, tranſported to other countries, may be of high importance. The following account is here given of the tree-butter. " The firſt town of note at which he arrived after leaving Sego, was called Kabba. It is ſituated in the midſt of a beautiful and highly cultivated country, bearing," ſays Park, " a greater reſemblance to the centre of England, than to what I ſhould have ſuppoſed had been the middle of Africa ;" and the ſeaſon was that of the ſhea-harveſt, or the gathering in the fruit which produces the ſhea-toulou, or tree-butter, the great abundance of which in this quarter was aſtoniſhing. The kernel is enveloped in a ſweet pulp, under a thin green rind; and it is Mr Park's opinion, that the butter produced from it, beſides the advantage of its keeping without ſalt the whole year, is whiter, firmer, and of a richer flavour, than the beſt butter he ever taſted from cows milk. The growth and preparation of this commodity ſeem to be among the firſt objects of African induſtry in this and the neighbouring ſtates; and it conſtitutes a main article of their inland commerce.

"The tree itſelf reſembles very much the American oak, and the nut, from the kernel of which the butter is prepared by boiling in water, has ſomewhat the appearance of a Spaniſh olive."

The following account is given of the lotus of ancient renown: "This is rather a thorny ſhrub than a tree, and it abounds in all the countries which Mr Park traverſed, but flouriſhes moſt in a ſandy ſoil. Its fruit is a ſmall yellow farinaceous berry, about the ſize of an olive, which, being pounded in a wooden veſſel, and afterwards dried in the ſun, is made into excellent cakes, reſembling in colour and flavour the ſweeteſt gingerbread. The natives of all deſcriptions eſteem it highly, and ſome of them prepare from it a liquor deliciouſly ſweet, the ſame, perhaps, which is fabled to have produced ſuch extraordinary effects on the companions of Ulyſſes.

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Dr Garnet of Glaſgow, in a letter to Dr Duncan, has communicated to him the following particulars reſpecting the oxygenated muriate of potaſh.

"In the obſervations on the oxygenated muriate of potaſh, which I formerly ſent you, and which were printed in the ſecond volume of your Annals, I hinted, that it might be an uſeful remedy in typhus, a diſeaſe in which there ſeems to be an unuſual deficiency of oxygen. Since the publication of theſe remarks, I have had an opportunity of adminiſtering this ſalt in ſeveral caſes of typhus, and always with ſucceſs.

"I begin with dozes of three or four grains in a little water, repeated three or four times a-day. I have never exceeded five grains; though a greater quantity may probably be given with ſafety. An increaſed flow of urine generally follows, often accompanied by one or two looſe ſtools. The tongue grows cleaner, the heat of the ſkin diminiſhes, and in the courſe of a few days the violence of the diſeaſe gradually abates. Beſides the trials which I have myſelf made with this ſalt, an eminent ſurgeon in this city, who attended ſome of the patients along

long with me, informs me, that he has likewiſe adminiſtered it in ſeveral caſes with the beſt effects.

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The practitioners in thoſe parts of England where the diſeaſe known by the name of the Cow-pox is obſerved, are not altogether agreed, as to the influence of that diſeaſe, in preventing the action of the variolous infection on the human body. An ingenious gentleman, at preſent a ſtudent of medicine at Edinburgh, has lately received a letter from an eminent practitioner in England, which contains the following paragraph.

"I have lately had a letter from a friend, who mentions two inſtances, from unqueſtionable authority, of perſons who, after having had what is called the Cow-pox, received the ſmall-pox by inoculation, as well as others. Theſe caſes appeared to him ſo ſtrong, that he wrote to Dr Jenner, ſtating them. He felt it more his duty to do this, as the opinion ſeems to be gaining ground, without any perſon's having taken the trouble of inquiring into the correctneſs of the fact; which, from this account, has probably been too haſtily adopted."

This ſubject has lately engaged the attention of Dr Pearſon of London, who has collected and publiſhed many facts from different quarters, tending, on the contrary, to eſtabliſh Dr Jenner's opinion, and to ſhew that the cow-pox may ſuperſede or extinguiſh the ſmall-pox.

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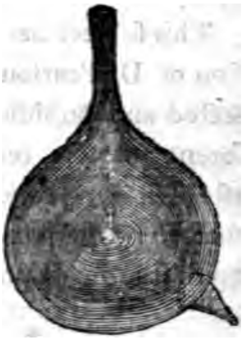
Dr Withering of Birmingham, in a letter to Dr Duncan, gives him the following account of a convenient method of inhaling the vapour of volatile ſubſtances.

" Having repeatedly had occaſion to obſerve the reſpiration of aſthmatic patients relieved by the vapour of radical vinegar, eſpecially in that ſtage of the paroxiſm in which expectoration is diſpoſed to take place; I wiſhed to contrive a method of inhaling it, which ſhould regulate the quantity taken into the lungs, and ſhould not require any unpleaſant exertion on the part of the patient.

Both theſe purpoſes I find to be perfectly anſwered, by a glaſs-veſſel reſembling what the chemiſts call a tubulated receiver, with a conical tube from the ſide of it, towards the bottom, ſuch as is repreſented in the margin. The globular part is 3½ inches in diameter; the cylindrical neck 3 inches long, and 1 inch diameter, open at the end; the conical tube about 1½ inch long, with an aperture of ¼ of an inch.



"If four or five drops of radical vinegar are put into this veſſel, which may be held in the hand, and the lips applied more or leſs cloſely to the mouth of the cylindrical neck, the vapour will paſs into the lungs, together with the air which is drawn in through the aperture of the conical tube.

"When it is deſireable that a very ſmall quantity only of the vapour ſhould be drawn in with the breath, the aperture of the conical tube may be cloſed by the forefinger of the hand which holds the glaſs, and the lower lip only may be brought into contact with the mouth of the cylindrical neck.

If the inſide of the glaſs be wetted with a ſolution of camphor in ſpirit of wine, the ſpirit ſoon evaporates, and leaves a coating upon the glaſs; and as the warmth of the hand is ſufficient to volatiliſe the camphor, it is evident that its vapour may thus be readily conveyed into the lungs; and I have found it uſeful in the catarrhus ſenilis. The vapour of æther, of aqua ammoniæ, and that of ſpirit of hartſhorn, riſe with very little heat; but if any ſubſtance be uſed which requires more heat than the hand can ſupply, the globular part of the veſſel may be made to reſt upon a cup of hot water, and thus the eſſential oil of peppermint, and other things capable of riſing in diſtillation, are eaſily volatiliſed.

Permit me to add a few remarks on the methods uſed to convey warm watery vapours into the lungs, becauſe I have found them very inadequate to the end propoſed; though I am ſatisfied that ſuch vapours may be uſed to advantage in caſes of peripneumony, and in recent catarrhs.

The late Mr Mudge, having obſerved the inefficacy of the means employed for this purpoſe, invented and publiſhed an account of an ingeniouſly contrived machine, which he called an INHALER, by means of which the air to be inſpired was intended to be drawn through a body of hot water. The idea is good, but it cannot be practically followed; for let any man carefully cloſe the joints of the machine, ſo as to guard againſt deception, and ſo that the air to be inſpired muſt really paſs through the body of water, and he will find upon trial, that the preſſure of the column of water, though only a few inches high, gives ſo much reſiſtance to the paſſage of the air, as to render this mode of breathing very laborious and troubleſome, even to perſons in health and ſtrength, and utterly impracticable to invalids.

I flatter myſelf that the following method of breathing watery vapour will be found unexceptionable. Partly fill a large jug, or other convenient veſſel, with hot water, and place it ſo that the riſing vapour may meet the mouth of the patient. Put it into a few lumps of quick-lime, which will preſently occaſion the water to boil, and to throw out its vapour copiouſly. By adding a little more lime from time to time, the proceſs may be continued at pleaſure, and the inſpired vapour will be more or leſs hot, and more or leſs denſe, according to the diſtance of the jug from the mouth of the patient.

It is but ſeldom that we ſee much typhus in Birmingham. The uſe of the nitrous vapour, in every inſtance of its adoption, ſtopped the further progreſs of infection, ſo that I am perſuaded we are much obliged to Dr C. Smyth on this occaſion.

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Mr James Anderson ſenior, ſurgeon in Edinburgh, has of late repeatedly uſed large doſes of calomel in caſes of Cynanche Trachealis, with the beſt effects. Theſe caſes, as ſoon as he can find ſufficient leiſure, it is his intention to prepare for publication, and they may perhaps be preſented to the public in ſome future volume of this Work. But whether ſuch a publication ſhall take place or not, we may at preſent obſerve, that from what Mr Anderſon has ſeen in his own practice, he is led to conclude, that calomel will be no leſs ſucceſsful in caſes of cynanche trachealis in Britain, than it has been repreſented to be in America, where it has long ſince been ſtrongly recommended in that affection.

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THE following official publication deſerves notice, as characteriſing the medical practice in the armies of the French Republic.

Medicis, Chirurgis, et Pharmacopœis caſtrenſibus, ſalubre militarium noſocomiorum conſilium.

Impetum moderantur, excitant torporem, aut vires victu ſuſtinent curaturi morbos. At quibus hæc inſtrumentis perficiunt? Iis, quæ copiam minuunt, acria molliunt, diluunt ſpiſſa, tenuata cogunt, ſtringunt laxa, laxant adſtricta, impetum in innoxia derivant loca, vel ſopiunt quoties ſedandum, ſed his, quæ tenuendo ſtimulant fibras, ſi languenti opitulandum naturæ erit.

Aqua, acetum, vinum, hordeum, nitrum,

mel, rheum, opium, ſtibium, ferrum, et ignis huic ſcopo ſufficiunt.

Candore ſolito exclamat Sydenhamus: "Agendi gnaro raram medendi penuriam." Quid ergo opus tam vaſta pompoſæ oſtentationis materie medica? Eo pulchrius officium fecit medicus, quo pauciora preſtitit. Sic ſe geſſerunt in artis exercitatio viri, qui laudantur, Hippocrates et Sydenhamus. Gradus perfectionis faſtigio proximus, apicem ſimplicitatis ſimul in arte detegit.

En collegæ amantiſſimi! Compendium pharmaciæ, viris, quibus neceſſitas belli incumbit et morbi, pro marte noſtro, accommodatum.

Id votum eſt, ut efficaciorum ad caſtrenſes caſus remediorum, cito, facili, et uniformi methodo parandorum tuto, nec injucunde ſumendorum, ſemper, et ubique, præſto eſſet copia. Curavimus (tempus poſtulabat et res), ut a formulis omnis ſupervacanea luxuriantis, vel etiam elegantioris pharmaciæ, aut farrago, aut ſupellex abeſſent; in indice vero nullum deſideraretur adimplendæ cujuſvis indicationis pro re nata, medium.

Selectis magis, quam numeroſis annuimus. Brevitatis et ſimplicitatis curioſi, in id etiam omnem diligentiam contulimus, ut quidquid antiquatum, inutile, aut elementorum repugnantia abſurdum, in abrogatis pharmacopœorum codicibus obrepſiſſet, hinc penitus reſcinderetur. Sic dum confectiones venerandæ antiquitati, unam et alteram conceſſimus, quot omnis generis obſoletæ pharmaciæ deliramenta, oblivioni, re ad lancem perpenſa, et bonis ægrorum rebus conſulentes, mandavimus, commiſimus :—Sic chemiam in conſilium advocavimus, non ampliandi, quin potius emendandi et reſtringendi codicis cauſa.

At dum junioribus collegis non formulas præfiniſſe, ſed formularum exemplaria proponere jubebat officii ratio ; a ſenioribus, et in praxi jamdudum verſatis, quicquid ipſis obtulit, quidquid detexit, aut deteget experientia, expectavimus. Quando autem in ancipiti et arduo epidemicorum ſtatu interroganda eſt natura, et quaſi remediis tentanda, eo quod huic vel alteri obſurdeſcat ; ſit jus cuivis medico et chirurgo eligendi, quod loco, tempori, et ægro magis accommoda-

tum judicaverit! Sua cuique eſto methodus! Libera, plane libera ſit artium liberaliſſima ars medendi!

Sed alta mente repoſtum habeat quiſquis de patria, et medicina caſtrenſi bene mereri cupit: Naturas eſſe medicatrices morborum. Et ſummam artis—Multa ſcire, et pauca agere.

Coſte, *Daignan*, *Louis*, *Dezoteux*, *Bayer*, *Parmentier*,——

Brion, *Sec.*

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Dr Cappe of York, in a letter to Dr Duncan junior, gives the following account of his experience of the effects of the nitrate of ſilver.

"In one caſe of angina pectoris, the internal uſe of the nitrate of ſilver removed all the ſymptoms. In two caſes of dyſpnœa, where paroxyſms of increaſed difficulty of reſpiration occurred every morning about two o'clock, this medicine had a powerful and ſpeedy effect. Both the patients were women, one aged forty-ſeven, the other ſixty-four. They had been many years ſubject to dyſpnœa, and winter-cough, and had been occaſionally relieved by antiſpaſmodics. The elder had taken conſiderable quantities of æther and opium.

" Theſe caſes came under my care in winter, a ſeaſon of the year which they had experienced to be very unfavourable to their complaints. One of theſe women I ſaw yeſterday; ſhe is now well. She walked about a mile to ſee me, without bringing on any difficulty of breathing, though we have lately had much cold and wet weather.

" I gave the nitrate of ſilver to a man who had been attacked with epilepſy ſoon after his marriage, and had ſuffered from the diſeaſe more than twenty-five years. The effect of this medicine was greater than could reaſonably have been expected. For a long time there had not been an interval of more than ten days between his fits. He firſt took the medicine on the day after he had many fits. He took a quarter of a grain thrice a-day, and had no fit for a month. He afterwards enjoyed an interval of five weeks. But though the intervals became gradually longer, I could not aſcertain that his fits were leſs violent.

"In a caſe of hyſteria, I have uſed the nitrate of ſilver, with ſome apparent benefit. The patient lives in the country, is irregular in her attendance on me, and probably alſo irregular in taking medicines. But after taking the nitrate of ſilver, ſhe was free from her diſorder for three weeks longer than uſual. She then had warning, as ſhe imagined, of an approaching paroxiſm, brought on by domeſtic vexations. The fit, however, was prevented by antiſpaſmodics, and I have not heard that ſhe has had any return of her diſorder ſince, though many weeks have elapſed.

"Dr Sims, in a paper read before the Medical Society of London, on the uſe of nitrate of ſilver in epilepſy, mentions, that it ſometimes aggravated the diſeaſe at firſt, though it afterwards effected a complete cure. I have not obſerved ſuch an effect, though I have ſeen the nitrate of ſilver uſed in many epileptic caſes, with more or leſs advantage. I am ſorry I cannot give you the general reſult of a greater number of caſes, as thoſe I allude to were patients at the Cary Street Diſpenſary in London; which town I left while the events were ſtill dubious.

"This medicine in general keeps the bowels open, it ſometimes produces diarrhœa, but it has not occaſioned any painful effect in any caſes under my obſervation, though given in the quantity of half a grain three times a-day. Boerhaave, however, in ſpeaking of a ſimilar preparation, ſays, 'Cavendum a nimio uſu, a nimis larga doſi cavendum, rodit ſemper et labefactat ventriculum imprimus. Quod vitium rob juniperi baccarum ſanat.'

"The effect of this medicine in the caſes I have mentioned, cannot, I think, be attributed to its apperient operation. For, in the caſe of angina pectoris, it was frequently neceſſary to give aloetic pills, to prevent coſtiveneſs. No other remedies were employed with the nitrate of ſilver, except in the caſe of hyſteria. In this inſtance it was neceſſary to unite opium with it, to prevent its purgative effects.

"Some experiments which I made on

frogs in the year 1793, ſuggeſted to me the thought of trying the nitrate of ſilver in ſome of the caſes which I have mentioned, after I had ſeen it uſed with much ſucceſs, by your father, in the Royal Infirmary. Theſe experiments, too, ſuggeſt ſome ſpeculations on the manner in which it operates; but you will, I dare ſay, readily excuſe me troubling you with them. Our friend, Dr Peſchier, in a letter dated, Vienna, 2d June 1798, writes me, that he had mentioned to one of the phyſicians of the hoſpital there, that the argentum nitratum had lately been employed with advantage, in Great Britain, in caſes of epilepſy. The phyſician tried it, and gave it to the extent of nine grains, to eight or nine ſuch patients, who, after a treatment of four weeks, without any effect, at length had their paroxiſms leſs frequent, and leſs violent.

"From the effects of this medicine, in caſes which have fallen under my obſervation, I think it has undeſervedly gone out of notice. But it would require no ſmall ſhare of credulity, to believe what is aſſerted of its virtues in Bates's Diſpenſatory. He ſays, ‘ It is a ſpecific for all diſeaſes of the head and brain ; as inveterate headachs, megrims, apoplexies, lethargies, vertigoes, epilepſies, palſies, &c. ; and is a famous thing in dropſies, and purges the brain from ſuperfluous humours.’ Overloaded with ſuch a catalogue of virtues, can one wonder it ſhould ſink into diſrepute?

“ As medical news, I may add, that Dr Ferriar of Mancheſter has found the nitrous acid very uſeful in the latter ſtages of typhus, when the Peruvian bark diſagrees, or when the patient will not take bark or wine. He gives half a dram three times a-day.

“ Baynton’s method of treating ulcers has proved ſucceſsful, to a wonderful degree, in Mancheſter. Both there, and at Liverpool, the oxygenated muriate of potaſh has been given in much greater doſes than thoſe ventured on by Mr Cruickſhank. Dr Currie gives fifteen grains, increaſing it according to its effects. Sometimes it ſeems as inactive as any other neutral ſalt ; but at other times it very much increaſes the heat of the body.”

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Dr Robert Jackson, already well known to the medical world, by his valuable Treatiſe on the Fevers of Jamaica, has been for ſome time engaged in preparing for the preſs, another Work on nearly the ſame ſubject. This publication will, we are told, be entitled, An Outline of the Hiſtory and Cure of Fever, Endemic and Contagious, with an explanation of the principles of military diſcipline and economy, and a ſcheme of medical arrangement for the armies.

Since the publication of Dr Jackſon's former Work, he has, for a conſiderable length of time, been engaged in a moſt important ſervice to his country; the ſuperintendance of the military hoſpitals in the iſland of St Domingo, where many of our brave ſoldiers fell victims to the climate. In the preſent Work, we may naturally expect to find the reſult of candid and extenſive experience, delivered with accuracy and fidelity. And we have no doubt that this publication will contain information of the greateſt importance to every practitioner, but eſpecially to thoſe who may be appointed to ſerve their country, in ſituations ſimilar to thoſe which Dr Jackſon has lately occupied.

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Dr Beddoes of Briſtol has circulated an advertiſement, announcing his intention of publiſhing a volume about the end of the preſent year, under the title of Contributions to Phyſical and Medical Knowledge, principally from the weſt of England and Wales.

A wiſh to ſave for the public obſervations calculated to enrich medicine, ſuggeſted the undertaking here announced. In furtherance of this wiſh, it occurred, that occaſional correſpondence, perſonal intercourſe, and facility of tranſmiſſion, might create in favour of a weſt-country publication, an intereſt which the Edinburgh and London collections do not every where excite. Hitherto the plan has been mentioned only to ſuch perſons as were known to have immemediate communications in their power. By all ſuch it has been approved;—by ſome, very warmly,—by others, ſo far as to induce them to grant their aid. Hence, the editor conſiders himſelf as nearly ſecure of a volume for 1798, and another for 1799. For future years the ſupply muſt chiefly depend on the inclination which ſhall be produced, at the outſet, among profeſſional and philoſophical men.

It is not propoſed to confine the Work ſtrictly to medical papers. With the philoſophy of inanimate matter, which bears in ſo many points upon this art, no practitioner of medicine ſhould be unacquainted. Nor does any thing ſeem more demonſtrable, than that every liberally educated individual ſhould be initiated in the philoſophy of animated nature. An extenſive ſeries of experiments in the firſt volume, will furniſh a ſtriking example of connection between theſe branches of knowledge; and the utility of keeping them together ought, on all occaſions, to be held up to contemplation.

It is the wiſh of the editor, that the profits (if any accrue) ſhould go to a public purpoſe.

poſe. As the inſtitution for inveſtigating the medicinal powers of factitious airs, will very ſpeedily be ſet on foot, and as the ſubſcriptions ſcarcely form a fund adequate to that great and difficult object, it is propoſed, that the profits of the two firſt volumes ſhould be deſtined to its enlargement, and that, afterwards, they be given to ſome infirmary within the diſtrict. The particular infirmary to be determined each year by lot. Communications may be addreſſed to Dr Beddoes, Phyſician, or to Mr Cottle, Bookſeller, Briſtol.

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Dr Garnet of Glaſgow has circulated propoſals for publiſhing, by ſubſcription, a Work which has no remote connection with medicine. It will be entitled, Obſervations on a Tour through the Highlands, and part of the Weſtern Iſles of Scotland, particularly Staffa and Icolmkill, containing a deſcription of the Country, Manners, and Cuſtoms of the Inhabitants, Agriculture, Fiſheries, Natural Curioſities, Mineralogy,

neralogy, Botany, Natural Advantages, Propoſed Improvements, &c. To which will be added, a Deſcription of the Falls of the Clyde, of the Country round Moffat, and an Analyſis of its Mineral Waters. By T. Garnet, M. D. Member of the Royal Medical, Phyſical, and Natural Hiſtory Societies of Edinburgh, the Literary and Philoſophical Society of Mancheſter, the Medical Society of London, the Royal Iriſh Academy, and Profeſſor of Phyſics and Philoſophy in Anderſon's Inſtitution, Glaſgow.

This Work will be illuſtrated by an accurate and elegant map, and about fifty plates engraved in the manner of aquatinta, and tinted from drawings taken upon the ſpot by Mr Watts, miniature and landſcape painter, who accompanied the author in the Tour. It will be compriſed in two large volumes 4to, price two guineas, to be paid on delivery of the Work. The names of ſubſcribers will be printed. As the expence of a Work of this kind is conſiderable, it was thought proper to aſcertain in this manner the number of copies it will be neceſſary to print off.

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In our laſt volume, we gave ſome account of Dr Thornton's intended Illuſtration of the Sexual Syſtem of Linnæus. In this Work, Dr Thornton has, we are informed, made very conſiderable progreſs; and from a numerous liſt of the moſt reſpectable ſubſcribers, whoſe names he has ſubjoined to a ſhort addreſs to the public, it would appear that the undertaking meets with the encouragement which he deſerves.

The following extract from the addreſs will ſhew upon what grounds the author expects encouragement in this Work.

" Dr Thornton flatters himſelf, that his new illuſtration of Linnæus, the plan of which was commenced during peace, will meet, even at the preſent time, (January 1ſt, 1798), with the foſtering protection of his country; and that, while the laurel is entwined round the brow of the warrior, the polite arts will not be allowed to ſicken and periſh for want of due ſupport, but that both will be equally cheriſhed by a great and opulent nation, which, while it brandiſhes the lightning of death, in defence of its independence, its liberties, and its honour, knows at the ſame time how to reſpect and cultivate the liberal arts, which add no leſs to the glory of a nation.

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MR BENJAMIN BELL, whoſe intended publication of Conſultations and Obſervations on various important points in Surgery, we mentioned in a former volume, has not yet put his Work to the preſs, but we underſtand that the manuſcript is very nearly finiſhed.

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MR JOHN BELL, ſurgeon in Edinburgh, already well known to the public by his former publications on Anatomy and Surgery, which we have repeatedly had occaſion to mention in the Medical Commentaries, has been for a conſiderable time engaged in preparing for the preſs a large Work on ſurgery, which, we are told, will probably extend to two volumes in quarto.

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Mr Andrew Bell, who has lately publiſhed the Third Part of his Anatomia Britannica, intended to illuſtrate the Anatomy of the Human Body, by upwards of three hundred copperplates, is buſily engaged in the farther proſecution of that great Work; and it is ſuppoſed that the Fourth and Fifth Parts will be publiſhed during the year 1799.

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Mr John Thomson, ſurgeon in Edinburgh, who has been for ſome time engaged in making a new tranſlation from the French of Fourcroy's Elements of Chemiſtry, has lately publiſhed the firſt volume of his tranſlation, which is enriched with many extenſive and important notes and references to the beſt ſources of information on each article. Mr Thomſon has, we are informed, made ſuch progreſs in his tranſlation, that the remaining volumes, which will alſo contain whatever has been made known ſince the publication of the original, will probably be publiſhed during the courſe of this winter.

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Mr Andrew Fyfe, the ingenious and induſtrious Aſſiſtant of Dr Monro, Profeſſor of Anatomy at Edinburgh, will ſpeedily publiſh an Anatomical Work, conſiſting of upwards of one hundred and fifty plates in quarto. A great part of them are executed by himſelf, in aquatinta, and diſtinguiſhed by colours. They will, we are perſuaded, prove of very great uſe to the Anatomical ſtudent. Moſt of them are copied from the beſt authors, others from original drawings taken from Mr Fyfe's own diſſections; and, from what we have ſeen, they appear to us to be excellently calculated for giving a very diſtinct view of the ſtructure of the different parts.

Mr Fyfe will ſoon alſo publiſh upwards of twenty other Anatomical Plates in folio, many of which were ſelected by the late Dr Monro, and which have ever ſince his death been in the poſſeſſion of his ſon, the preſent Profeſſor of Anatomy. Theſe Plates were engraved by the firſt artiſts of that period in Edinburgh, ſome of them by the late Sir Robert Strange, who then reſided here. This Work will alſo, we are perſuaded, be of conſiderable uſe to the induſtrious ſtudent. The Figures, in general, appear to us to be judiciouſly choſen; and the Explanations, which have been made out by Mr Fyfe, have been rendered as conciſe as the nature of ſuch a Work will allow.

C. Fourcroy of Paris is about to publiſh a Work, entitled, Chemie Medicale. The ſame excellent chemiſt, in company with Mr Vauquelin, is preparing a general and complete Hiſtory of Vegetable Analyſis.

C. Desfontaines, the National Profeſſor of Botany, is at length publiſhing his Plants of Barbary, with plates. The Work will contain deſcriptions of fifty or ſixty new ſpecies.

Mr Scherer is preparing for the preſs, a General Hiſtory of Chemiſtry, and alſo an Encyclopædic Dictionary of the ſame Science.

Dr Bloch, the celebrated ichthyologiſt, of

Berlin, is about to publiſh a Compendium Ichthyologicum.

Profeſſor Soemmering is mentioned as being about to publiſh in Leipſick, a Treatiſe on the Human Organs of Senſe. It is to contain twenty-four plates, twelve of them outlines. The editors promiſe that the engraving, paper, and type, will correſpond with the importance and excellence of the Work.

Profeſſor Barton of Philadelphia is preparing for the preſs, a Work entitled, Strictures on the Arrangement of Materia Medica, adapted by Dr Darwin.

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ON the 11th of December 1797, died at London, Dr Richard Brockleſby, a phyſician of great eminence. In a future volume, we ſhall probably preſent our readers with ſome biographical anecdotes of this diſtinguiſhed character. At preſent we muſt confine ourſelves to what we believe is a correct liſt of his publications.

Diſſertatio inauguralis de Saliva Sana et Morboſa. Lugd. Bat. 4to. 1745.

An Eſſay concerning the Mortality of Horned Cattle. 8vo. London, 1746.

Elogium Medicum; ſive, Oratio Anniverſaria Harveiana, habita in Theatro Collegii Regalis Medicorum Londinenſium, die 18mo Octobris 1760. 4to. London, 1761.

Economical and Medical Obſervations, from 1738 to 1763, tending to the improvement of medical hoſpitals. 8vo. London, 1764.

An Account of the Poiſonous Root lately found mixed with the Gentian. Philoſophical Tranſactions.

Caſe of a Lady labouring under Diabetes. Medical Obſervations, vol. III.

Experiments relative to the Analyſis and Virtues of Seltzer Water. Ibid. vol. IV.

Caſe of an Encyſted Tumour in the Orbit of the Eye, cured by Meſſ. Bromfield and Ingram. Ibid. vol. IV.

A Diſſertation on the Muſic of the Ancients.

On the 17th of January 1798, Dr Thomas Kirkland, an eminent medical practitioner,

tioner, died at Aſhby de la Zouch. His different publications on important ſubjects, will afford a laſting teſtimony of his genius and induſtry. Among theſe we may mention the following, as juſtly entitling him to a high degree of reputation.

A Treatiſe on Gangrene, in which the caſes that require the uſe of the bark, and thoſe in which it is pernicious, unleſs joined with proper correctives, are aſcertained, and the objections to its efficacy in the cure of gangrene, conſidered. 8vo. 1754.

An Eſſay on the Methods of Suppreſſing Hæmorrhage from divided Arteries. 8vo. 1763.

An Eſſay on the Improvement in the Cure of theſe Diſeaſes, which are the cauſes of fevers. 8vo. 1767.

A Treatiſe on Childbed-Fevers, and of the Methods of preventing them. 8vo. 1774.

An Inquiry into the preſent ſtate of Medical Surgery, including the Analogy between external and internal Diſorders. 8vo. Vol. I. 1783. Vol. II. 1786.

A Commentary on Apoplectic and Paralytic Affections, and on the Diſeaſes connected with the Subject. 8vo. 1794.

On the 17th of February 1798, Francis Geach, M. D. F. R. S. died ſuddenly at his houſe at Plymouth. Dr Geach had been about twenty years ſurgeon to the Royal Naval Hoſpital, and had diſcharged the duties of that office with great credit to himſelf. He was the author of ſeveral ingenious and intereſting publications, among which may be mentioned, The Caſe of a Man who had ſix ſtones taken out of the gall-bladder. Philoſophical Tranſactions, vol. 53. The Caſe of a Man wounded in the Left Eye with a ſmall Sword; Ibid. Medical and Chirurgical Obſervations on Inflammations of the Eye, Ulcers, and Gunſhot Wounds. 8vo: 1766. Some Obſervations on Dr Baker's Eſſay on the Endemical Cholic of Devonſhire. 8vo. 1767.

On the 18th of June 1798, died at Brampton Row, Knightſbridge, Dr John Aſh, Fellow of the Royal and Antiquarian Societies, and of the Royal College of Phyſicians of London. Dr Aſh obtained the degree of Doctor of Medicine from the Univerſity of Oxford in 1754. He practiſed for many years as Phyſician at Birmingham, where he deſervedly obtained very high reputation.

On the 28th of July, died at Edinburgh, James Clark, ſurgeon, an ingenious and amiable young man.

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STATE of the Thermometer, Barometer, and Rain, during the year 1797, according to Obſervations made about a mile from the City of Edinburgh.

MONTH.	THERMOMETER.			BAROMETER.			RAIN.
	High. Deg.	Low. Deg.	Med. Deg.	Higheſt Inches.	Loweſt Inches.	Med. Inches.	Inches & Decimals
Jan.	55	32	42	30.30	28.87	29.70	1.350
Feb.	59	39	50	30.50	28.90	29.93	0.395
Mar.	57	38	47	30.25	29.20	29.76	1.200
April	60	41	50	30.20	29.16	29.61	1.612
May	74	46	57	30.00	29.00	29.55	2.530
June	66	49	58	29.96	29.15	29.65	2.085
July	74	55	63	30.00	29.22	29.61	4.558
Aug.	69	58	62	29.84	29.05	29.48	3.820
Sept.	63	54	58	29.84	28.86	29.43	2.655
Oct.	61	41	51	30.03	28.95	29.60	4.920
Nov.	56	32	44	30.20	28.85	29.65	1.025
Dec.	53	26	42	30.05	28.65	29.40	2.816
Whole Year.	74	26	52	30.5	28.65	29.62	28.960

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STATE of the Thermometer, Barometer, and Rain, during the year 1797, according to Obſervations made at the Apartments of the Royal Society of London.

MONTH.	THERMOMETER.			BAROMETER.			RAIN.
	High. Deg.	Low. Deg.	Med. Deg.	Higheſt Inches.	Loweſt Inches.	Med. Inches.	Inches &c. Decimals
Jan.	49	25	37	30.50	29.52	30.09	0.960
Feb.	50	24	37	30.62	29.37	30.31	0.219
Mar.	54	29	40	30.42	29.44	29.94	0.777
April	65	35	47	30.13	29.10	29.77	1.859
May	78	40	55	30.33	29.38	29.89	1.436
June	73	45	58	30.29	29.36	29.86	4.223
July	84	55	66	30.25	29.51	29.96	1.288
Aug.	76	52	62	30.18	29.48	29.87	2.789
Sept.	69	45	57	30.14	29.04	29.75	4.061
Oct.	62	35	49	30.31	29.05	29.83	2.001
Nov.	57	27	43	30.48	29.14	29.92	1.473
Dec.	56	30	43	30.46	29.07	29.80	1.611
Whole Year.	78	24	50	30.62	29.04	29.92	22.697

During the year 1798, Sir John Sinclair Bart. Dr Robert Kennedy, Dr Alexander Monro, jun. the Right Honourable Lord Daar, Lieutenant-Colonel Imrie, and James Bonnar, Eſq. have been admitted Reſident-members of the Royal Society of Edinburgh; Dr David Doig, the Right Honourable the Earl of Traquair, and William Drummond, Eſq. have been admitted Non-reſident Members.

During the ſame year, Dr Colin Lauder has been elected a Fellow, and Dr George Gavin Browne of Bath has been admitted a Licentiate, of the Royal College of Phyſicians of Edinburgh. Mr John Herdman and Mr George Bell have been admitted Members of the Royal College of Surgeons.

On the 15th December 1798, Dr Alexander Monro jun. was admitted Profeſſor of Anatomy and Surgery in the Univerſity of Edinburgh, in conjunction with his Father, Dr Alexander Monro ſen. who was firſt admitted into that office, on the 12th of July 1755.

SECT. IV.

LIST OF NEW BOOKS.

THE Natural Hiſtory of the rarer Lepidopterous Inſects of Georgia; including their Syſtematic Characters, the particulars of their ſeveral Metamorphoſes, and the Plants on which they feed. Collected from the Obſervations of Mr John Abbot, many years reſident in that country, by James Edward Smith, M. D. F. R. S. & A. S. Two volumes folio, and 104 plates. London, 1797.

A collection of Weſt-India Exotics from Antigua; with appropriate deſcriptions, 12 Plates, royal folio. London, 1798.

Eſſays on the Microſcope by the late George Adams, the 2d edition, with conſiderable additions and improvements. By Frederick Kanmacher, F. L. S. 4to. 32 Plates. London, 1798.

The Natural Hiſtory of the Year; being an enlargement of Dr Aikin's Kalendar of Nature. By Arthur Aikin. 8vo. 1798.

A Few Facts and Obſervations on the Yellow Fever of the Weſt Indies; in which it is ſhewn, that there have exiſted two ſpecies of Fever in the Weſt-India Iſlands for ſeveral years paſt, indiſcriminately called Yellow Fever, but which have proceeded from very different Cauſes. With the Succeſs attending the Method of Cure. By James Anderſon, Fellow of the Royal College of Surgeons of Edinburgh, and late Surgeon to the 60th Regiment. 8vo. Edinburgh, 1798.

An Eſſay on Population, as it affects the Future Improvement of Society; with remarks on the Speculations of M. Godwin, Condorcet, and other Writers. 8vo. London, 1798.

An Inquiry into the Nature and Cauſe of Fever, more eſpecially the Intermitting;

containing an Inveſtigation into the Nature of Miaſma, and the Manner of its Action on the Human Body. 8vo. Edinburgh, 1797.

A Practical Synopſis of the Materia Alimentaria and Materia Medica. By the Author of the Theſaurus Medicaminum. Vol. I. 8vo. London, 1797.

Obſervations on the Effects of Oxygen on Animal and Vegetable Subſtances. Part I. By C. Archer, Eſq. 8vo. London, 1798.

The Seamen's Medical Advocate; an Attempt to ſhew, that 500 Seamen are annually, during the War, loſt to the Nation in the Weſt Indies through the Yellow Fever, and other Diſeaſes, from Cauſes which, it is conceived, are chiefly to be obviated. By Elliot Arthy, Surgeon in the Weſt-India Service. 8vo. London, 1798.

Aſiatic Reſearches; or, Tranſactions of the Society inſtituted at Bengal, for inquiring into the Hiſtory and Antiquities, the Arts, Sciences, and Literature of Aſia. Vol. IV. 4to, Calcutta;—and 8vo, London, 1798.

An Appendix to the firſt edition of the Morbid Anatomy. By Matthew Baillie, M. D. F. R. S. 8vo. London, 1798.

Collections for an Eſſay towards a Materia Medica of the United States. By Dr Barton of Philadelphia. 8vo. Philadelphia, 1798.

A Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattleſnake, and other American Serpents. By Benj. Smith Barton, M. D. and Profeſſor of Natural Hiſtory and Botany. 8vo. Philadelphia, 1796.

A Lecture, introductory to a courſe of Popular Inſtruction on the Conſtitution and Management of the Human Body. By Thomas Beddoes, M. D. London, 1798.

A Syſtem of Diſſection; explaining the Anatomy of the Human Body, the Manner of Diſplaying the Parts and their Varieties in Diſeaſe. By Charles Bell. Folio. London, 1798.

Anatomia Britannica; a Syſtem of Anatomy, in Six Parts. Illuſtrated by upwards of 300 folio Copperplates, from the moſt celebrated authors in Europe. By Andrew Bell, F. S. A. S. Engraver to his Royal Highneſs the Prince of Wales. Conducted by Andrew Fyfe, Aſſiſtant to Dr Monro, Prof.

of Anatomy in the Univerſity of Edinburgh. Part III. Edinburgh, 1798.

The Hiſtory of Britiſh Birds, with Figures engraved on wood. By T. Bewick. Vol. I. London, 1797.

An Eſſay on Electricity; explaining fully the Principles of that Science, and the moſt approved Inſtruments and Apparatus, both to illuſtrate the theory, and render the practice uſeful and entertaining. With a Letter on Medical Electricity. By Mr Birch. 4th edition. 8vo. With ſix plates. London, 1798.

Eſſays on the Venereal Diſeaſe, and its Concomitant Affections; illuſtrated by a variety of Caſes. Eſſay I. Part I. On the Anti-venereal Effects of the Nitrous Acid, Oxygenated Muriat of Potaſh, and ſeveral analogous Remedies, which have been lately propoſed as Subſtitutes for Mercury. By W. Blair, A. M. Surgeon of the Lock Hoſpital. 8vo. London, 1798.

The Soldier's Friend; or, the Means of preſerving the Health of the Military Men, who may be called into the Service of their Country in the preſent Criſis. By W. Blair,

A. M. Surgeon of the Lock Hoſpital. 8vo. London, 1798.

The Anatomy of the Horſe. By D. Blairec. With plates. Folio. London, 1798.

A Lecture on the Situation of the large Blood-veſſels of the Extremities, and the Methods of making Effectual Preſſure on the Arteries in caſes of dangerous Effuſions of Blood from Wounds. A new edition. To which is now added, An Explanation of the Nature of Wounds, more particularly thoſe received from Fire-arms. By W. Blizard, F. R. S. London, 1798.

Botanical Dialogues, between Hortenſia and her four Children, Charles, Harriet, Juliet, and Henry. Deſigned for the Uſe of Schools. By a Lady. 8vo. 15 plates. London.

Embaſſy from the Dutch Eaſt-India Company to China. From the Journal of E. van Braam. 8vo. London, 1798.

A Treatiſe on Scrophulous Diſeaſes, ſhewing the good effects of factitious airs. By C. Brown. 8vo. London, 1798.

Obſervations on the Zoonomia of Dr Darwin.

win. By Thomas Brown, Eſq. 8vo. Edinburgh, 1798.

Enchiridion Syphiliticum; or, Directions for the Domeſtic Treatment of Venereal Complaints. By A. P. Buchan, M. D. 8vo. London, 1798.

A Diſſertation on the Chemical and Medical Properties of the Briſtol Hot-well water. To which are added, Practical Obſervations on the prevention and treatment of Pulmonary Conſumption. By A. Carrick, M. D.

A Medical Sketch of the Synochus Maligna, or Malignant Contagious Fever, as it appeared in the city of Philadelphia. To which is added, ſome account of the morbid appearances obſerved on Diſſections; with a ſhort Chemical Analyſis of the Black Matter ejected from the Stomach in the laſt ſtage of the Yellow Fever; with its diſſimilarity to Putrid Bile, and its effects when applied to the healthy Syſtem. By Iſaac Cathrall. 8vo. Philadelphia, 1796.

Cautions to Women in the ſtate of Pregnancy. 8vo. London, 1798.

An Eſſay on the Medicinal Properties of

Factitious Airs, with an Appendix on the nature of the Blood. By Tiberius Cavallo, F. R. S. London, 1798.

Obſervations in Defence of a Bill lately brought into Parliament, for erecting the Corporation of Surgeons into a College, including a Sketch of the Hiſtory of Surgery in England. By Thomas Chevalier, A. M. 8vo. London, 1797.

Obſervations on the Structure, Economy, and Diſeaſes of the Foot of the Horſe, and on the Principles and Practice of ſhoeing. By Edward Coleman, Prof. of the Veterinary College. 4to. London, 1798.

An Inquiry into the Nature of Mental Derangement; comprehending a conciſe Syſtem of the Philoſophy and Pathology of the Human Mind; and a Hiſtory of the Paſſions and their Effects. By Alexander Crichton, M. D. Phyſician to the Weſtminſter hoſpital, and Lecturer on the Theory and Practice of Phyſic and Chemiſtry. 2 vols, 8vo. London, 1798.

Practical Obſervations on the Diſeaſe of the Joints, commonly called White Swelling; with ſome Remarks on Scrophulous Abſceſſes,

Abſceſſes. By Bryan Crowther, Surgeon to Bethlem and Middleſex.

Clinical Lectures, delivered in the year 1765 and 1766. By William Cullen, M. D. Taken in ſhort-hand by a gentleman who attended. 8vo. London, 1797.

Medical Reports on the Effects of Water, cold and warm, as a remedy in Fever and Febrile Diſeaſes, whether applied to the ſurface of the body, or uſed as a drink. With Obſervations on the nature of Fevers, and on the effects of Opium, Alcohol, and Inanition. 2d edit. corrected and enlarged. By James Currie, M. D. F. R. S.

Obſervations on the Cauſes and Cure of Remitting or Bilious Fever. To which is annexed, An Abſtract of the opinion and practice of different Authors; an Appendix, exhibiting facts and ſpeculations relative to the Synochus Icteroides, or Yellow Fever. By William Currie, Fellow of the College of Phyſicians of Philadelphia. 8vo. Philadelphia, 1798.

Conſtantinople, Ancient and Modern, with Excurſions to the ſhores and iſlands of the Archipelago, and to the Troad. By James Dallaway, M. B. F. S. A. late Chaplain and Phyſician of the Britiſh Embaſſy to the Porte. 4to. 10 plates. London, 1798.

A Plan for the Conduct of Female Education in Boarding-ſchools. By Eraſmus Darwin, M. D. F. R. S. 4to. Derby, 1797.

Annals of Medicine for the year 1797, containing a conciſe View of the lateſt and moſt important Diſcoveries in Medicine and Medical Philoſophy. By A. Duncan, ſen. and jun. M. D. F. R. S. E. Vol. II. 8vo. Edinburgh, 1798.

Eſſay on Viſion, explaining the fabric of the Eye, and the nature of Viſion, with proper Rules to know when ſpectacles are neceſſary, and the choice of them without injuring the ſight. 2d edit. 8vo. London, 1797.

Medical Hiſtories and Reflections. By John Ferriar, M. D. Vol. III. 8vo. London, 1798.

Caſes of the ſucceſsful Practice of the Veſicæ Lotura in the caſe of diſeaſed Bladders. By Jeſſe Foot, Surgeon. 8vo. London, 1798.

A third Diſſertation on Fever, part 1ſt, containing the hiſtory and treatment of a regular continued fever, ſuppoſing it is left to purſue its ordinary courſe. By G. Fordyce, M. D. F. R. S. 8vo. London, 1798.

The Economy of Nature, on the Principles of Modern Philoſophy. 2d edition, with conſiderable additions. By G. Gregory, M. D. 3 volumes, 8vo. 56 plates. London, 1798.

Remarks on Hydrophobia, or the diſeaſe produced by the bite of a mad dog, or other rabid animal, &c. By R. Hamilton, M. D. 2 vols, 8vo. London.

Some new Experiments, with Obſervations upon Heat, clearly ſhewing the erroneous principles of the French Theory. Alſo a letter to Henry Cavendiſh, Eſq. containing ſome pointed Animadverſions, with ſtrictures upon ſome late chemical papers in the Philoſophical Tranſactions, and other remarks. By Robert Harrington, M. D. 8vo. London, 1798.

Obſervations on Inſanity; with practical remarks on the diſeaſe, and an account of the morbid appearances on diſſection. By John Haſlam. 8vo. London, 1798.

Stricture on the Elementa Medicinæ of

Dr Brown. By Phinehas Hodges. 8vo. Goſhen, 1795.

Hints deſigned to promote Beneficence, Temperance, and Medical Science. Vol. I. 8vo. London, 1797.

The Flora of Germany; or, A Botanical Pocket-companion. By George Francis Hoffman. Vol. I. and II. with coloured plates. 8vo, or 12mo. Erlangen, 1791—96.

The Anatomiſt's Vade-mecum; containing a conciſe and accurate deſcription of the ſtructure, ſituation, and uſe of every part of the human body. To which is added, An explanation of Anatomical Terms. By Robert Hooper of Pembroke Hall, Oxford, M. D. F. L. S. 8vo. London, 1797.

A compendious Medical Dictionary. By R. Hooper, M. D. 12mo. London, 1798.

An Illuſtration of the Analogy of Vegetable and Animal Parturition. By A. Hunter, M. D. F. R. S. L. & E.

An Outline of the Mineralogy of the Shetland Iſlands, and of the Iſland of Arran. With copperplates, and obſervations on peat, kelp, and coal. By Robert Jameſon. 8vo. London, 1798.

An Inquiry into the Cauſes and Effects of the Variolæ Vaccinæ, a Diſeaſe diſcovered in ſome of the Weſtern Counties of England, particularly Glouceſterſhire, and known by the name of the Cow-pox; with Obſervations on the Origin of the Small-pox, and on Inoculation. By Edward Jenner, M. D. F. R. S. 4to. With plates, 1798.

A View of the Perkinean Electricity; or, An Inquiry into the Influence of Metallic Tractors, founded on a newly diſcovered Principle in Nature, and employed as a Remedy in many painful and inflamatory Diſeaſes, as Rheumatiſm, Gout, Quinſy, Plueriſy, Tumefactions, Scalds, Burns, and a variety of other Topical Complaints; with a Review of Mr Perkins' late Pamphlet on the Subject; to which is added, An Appendix, containing a Number of Experiments made in London, Bath, Briſtol, &c. with a view of aſcertaining the Efficacy of this Practice. By Charles Cunningham Langworthy, Surgeon of Bath, 8vo. Briſtol, 1798.

A Philoſophical and Practical Treatiſe on Horſes,

Horſes. By John Lawrence. 2 vols, 8vo. London, 1798.

The Select Works of Antony Van Leeuwenhoek; containing his Microſcopical Diſcoveries in many of the Works of Nature. Tranſlated by Samuel Hoole. Part I. With plates, London 1798.

Narrative of the ſhipwreck of the ſhip Juno, on the Coaſt of Africa, and of the Singular Preſervation of fourteen of the Crew on the Wreck, without food, during a period of twenty-three days. By William Mackay, late ſecond Officer of the Ship. 8vo. London, 1798.

An Inquiry into the Nature and Cauſes of the Great Mortality among the Troops of St Domingo. By Hector Maclean, M. D. 8vo. London, 1798.

Stapeliæ Novæ; or, A Collection of ſeveral new Species of that Genus diſcovered in the Interior Parts of Africa. By Francis Maſon. Small folio. 41 coloured plates. London, 1796.

Medical Reſearches, ſelected from the Papers of a private Medical Aſſociation.

Vol. I. Part I. 8vo. With plates. London, 1798.

Medical Review. Vol. IV. From May 1797 to May 1798. 8vo. London, 1798.

The Pupil of Nature; or, Candid Advice to the Fair Sex on the ſubject of Pregnancy, Childbirth, the diſeaſes incident to both, the fatal effects of ignorance and Quackery, and the moſt approved means of promoting the Health, Strength, and Beauty of their Offſpring. By Martha Mears, Practitioner in Midwifery. 12mo. London, 1797.

Memoirs of the Literary and Philoſophical Society of Mancheſter. Vol. V. Part I. With plates. 8vo. London, 1798.

Deſcription of the Minerals in the Leſkian Muſeum. By Karſten. Tranſlated by George Mitchell, M. B. Two volumes, 8vo. London, 1798.

An Appendage to the Toilet; an Eſſay on the Teeth. By Hugh Moiſes. M. D. London, 1798.

A Journal of Natural Philoſophy, Chemiſtry, and the Arts. By William Nicholſon. Vol. I. 4to. London, 1798.

A Treatiſe on Mortal Diſeaſes, containing

a

a particular view of the different ways in which they lead to death, and the beſt means of preventing them, by medical treatment, from proving fatal. By Conrad George Ontyd, M. D. 8vo. London, 1798.

An Account of Mr Park's Journey into the interior parts of Africa. From the proceedings of the African Aſſociation. 8vo. London. 1798.

An Inquiry concerning the Hiſtory of the Cow-pox, principally with a view to ſuperſede and extinguiſh the Small-pox. By George Pearſon, M. D. Phyſician to St George's Hoſpital. 8vo. London, 1798.

The Arguments in favour of an Inflammatory Diatheſis in Hydrophobia, conſidered; with reflections on the nature and treatment of the Diſeaſe. By Richard Pearſon, M. D. 8vo. London, 1798.

Phyſiology; or, An attempt to explain the Functions and Laws of the Nervous Syſtem, the contractions of Muſcular Fibres, and the conſtant and involuntary actions of the Heart, the Stomach, and Organs of reſpiration, by means of ſimple, univerſal, unvarying

ing principles, &c. By E. Peart, M. D. 8vo. London, 1798.

The Influences of Metallic Tractors on the Human Body, in removing various painful inflammatory Diſeaſes. By B. D. Perkins, A. M. ſon of Dr Perkins of North America, the diſcoverer. 8vo. London, 1798.

The Voyage of La Peyrouſe round the world, in 1785, 86, 87, and 88. Arranged by M. L. A. Millet Mureau. Tranſlated from the French. In 3 large vols 8vo, without omiſſions of any kind, and with nearly 50 plates. London, 1798.

Philoſophical Tranſactions of the Royal Society of London, for the year 1797, and Part I. for 1798. 4to. London, 1798.

Regulations to Regimental Surgeons, for the better management of the Sick in Regimental Hoſpitals. 8vo. London, 1798.

Repoſitory of the Arts and Sciences. Vol. VII.

Reports for bettering the Condition, and encreaſing the Comforts of the Poor. Six Numbers. 8vo. London, 1798.

Reflections on the Surgeons Bill, in anſwer to three pamphlets in defence of that Bill.

By John Ring, member of the Corporation of Surgeons. 8vo. London, 1798.

Caſes of Diabetes Mellitus, with reſults of the trials of certain acids and other ſubſtances in the cure of the Lues Venerea. By John Rollo, M. D. Surgeon-general, Royal Artillery. 2d edition, with large additions, 8vo. London, 1798.

Plants of the coaſt of Coromandel, ſelected from the drawings and deſcriptions preſented to the Honourable Court of Directors of the Eaſt-India Company. By W. Roxburgh, M. D. Publiſhed by their order, under the direction of Sir Joſeph Banks, P. R. S. Four Faſciculus. Elephant Folio. London, 1795. &c.

Count Rumford's Eſſays, Experimental, Political, Economical, and Philoſophical. Part II. Eſſay VII. 8vo. London, 1798.

Eſſay VIII. on the Propagation of Heat in various Subſtances.

Eſſay IX. An Inquiry concerning the Source of Heat, excited by Friction.

Medical Inquiries and Obſervations, containing an account of the bilious, remitting, and intermitting Yellow Fever, as it appeared in Philadelphia, in the year 1794. Together with an inquiry into the proximate cauſe of Fever; and a defence of Blood-letting, as a remedy in certain diſeaſes. By Benjamin Ruſh, M. D. Vol. IV. 8vo. Philadelphia, 1796.

A Treatiſe on the Teeth. B the Chevalier Ruſpini. 12mo. London, 1797.

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